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RESEARCH REVIEW

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RESEARCH BRIEFS

New Review Editor

Murray Blumenthal, Ph.D., recently appointed Director of Research for the National Safety Council, is the new editor of the RESEARCH REVIEW.

Dr. Blumenthal comes to the Council from Chico State College, Chico, Calif., where he was Assistant Professor of Psychology. Prior to that he was an Instructor in the Psychology Department of the University of Denver, where he earned his B.M.E., M.A., and Ph.D. in General Experimental Psychology.

The new editor is a member of the American Psychological Association and Sigma Xi, the national science society.

Dr. Blumenthal succeeds John J. "Mike" Flaherty, who was NSC's director of research for nearly four years before deciding to go into private business. Flaherty guided the RESEARCH REVIEW during most of its existence and selected the material in this issue.

From Our Readers

In future issues space will be set aside for letters from our readers. Particularly encouraged are letters commenting on articles appearing in the RESEARCH REVIEW. When possible, the editor will forward critical comments to authors so that the comments and author's reply will appear in the same issue.

Wanted: Reviews of New Material

The editor invites reviews of books, pamphlets and reports pertinent to traffic safety research. Space in the REVIEW will be made available for contributions of interest to our readers.

THE AUTHORS

J. D. Chastain

J. D. Chastain received a Bachelor's degree with a major in chemistry from Howard Payne College, Brownwood, Texas, in 1948. Mr. Chastain began his career in police work with the Texas Department of Public Safety in the same year, and attended the course for recruit Texas Highway Patrolmen at the Department of Public Safety Police Academy.

Upon graduation, He was transferred to the laboratory as a chemist and toxicologist, and served in that capacity for six years. In 1957, Mr. Chastain was made manager of the laboratory which put him in charge of the entire Crime Laboratory which is composed of six sections.

Mr. Chastain attended the seminar on Alcohol and Road Traffic conducted by the Southern Police Institute at the University of Louisville in 1956. He also attended the seminar on Chemical Test Supervision held at Indiana University in 1960.

Mr. Chastain is a member of The American Academy of Forensic Sciences and the National Safety Council's Committee on Alcohol and Drugs.

Richard F. Crowther

Mr. Crowther is a native of Toronto, Canada. He is a graduate of Pickering College. Newmarket, Ontario. He received his B.A. from Queens University, Ontario, in 1952. Following graduation he spent three years on the research staff in the Canadian headquarters office of the Prudential Insurance Co. in Toronto. Since 1955 he has been engaged in graduate work in economics at the University of Chicago, and is continuing his work there (in absentia) toward a Ph.D. degree. He joined the research staff of the Traffic Institute at Northwestern University in early 1959. Since September 1, 1959, he has been engaged in research with the Field Service Division of the International Association of Chiefs of Police, Washington, D. C.

Lt. Charles A. Davis

Lt. Davis is a career man of 19 years service with the Indiana State Police; 16 of these years have been devoted to specialized work in the laboratory services. His intense interest in biologic science, evident in undergraduate work at Indiana University, has become the basis for further study and the publication of numerous articles.

Lt. Davis, who has been director of the Indiana State Police Laboratory for three years is also a member of the Harvard Associates in Police Science, Chairman of the Criminalistics Section of the American Academy of Forensic Sciences, and Chairman of the Indiana State Commission on Forensic Sciences.

Robert B. Forney

Dr. Robert B. Forney was born July 9, 1916, in Ashley, Indiana. With the exception of three years in the Army of the United States, he has spent his entire life in Indiana. He received his A.B. degree from Indiana University in 1938 and the following year his A.M. degree. His Ph.D. degree was completed in 1948 in the Medical School at Indianapolis in the field of toxicology.

Dr. Forney is currently an Associate Professor of Pharmacology at Indiana University School of Medicine and directs the Indiana State Laboratory for Toxicology at the Medical School. His research interests have always been in the field of alcohol and more particularly, the behavioral effects of alcohol alone and in combination with other drugs.

He is a member of the National Safety Council's Board of Directors, Executive Committee on Traffic of the National Safety Council and the Committee on Alcohol and Drugs of the National Safety Council. Dr. Forney is a Fellow in the Academy of Forensic Sciences and a member of the Society for Experimental Biology in Medicine.

Francis W. Hughes

Dr. Francis W. Hughes was born April 20, 1920, in Plains, Pennsylvania. During World War II he served in the United States Navy as a pharmacist's mate. He received his B.S. degree in 1949 and his M.S. degree in 1951 from Temple University. His Ph.D. degree was completed in 1954 in the medical school at the University of Wisconsin in the field of Pharmacology.

Dr. Hughes is currently an assistant professor of Pharmacology at Indiana University School of Medicine. His research interests have been in the field of pathologic changes in the central nervous system as affected by enzyme changes, cardiovascular effects of metabolites of serotonin and behavioral alterations resulting from tranquilizers, sedatives and alcohol.

He is a member of the American Society for Pharmacology, Sigma Xi, and the Society for Experimental Biology and Medicine.

Harold R. Hulpieu

Dr. Harold R. Hulpieu was born in Dodge City, Kansas, February 21, 1901. He completed his A.B. degree at Southwestern College in Kansas in 1923. In 1924 he completed his Master's degree at Oklahoma University and was named a Bruce Fellow to Johns Hopkins University where in 1928 he was awarded a Ph.D. degree in Physiology. In 1928 he joined the faculty at Indiana University School of Medicine where he remained until his retirement in 1960 as Chairman, Department of Pharmacology.

Dr. Hulpieu has made significant contributions in the field of alcohol metabolism. He is the author of numerous papers and chapters on alcohol. Since his retirement, his address is Naples, Florida.

R. Dean Smith

Born in Salem, Ohio in 1928, Mr. Smith received a B.A. in Criminology (1956) from the University of California at Berkeley. While pursuing graduate studies at that school, he also served on the Oakland police force.

Before joining the staff of the Field Service Division of the International Association of Police Chiefs as a research assistant, Mr. Smith served in the same capacity with the Northwestern University Traffic Institute.

Alfred F. Mayerhofer

Mr. Mayerhofer is presently employed as Director of Safety for Sims Motor Transport Lines of Chicago. In this capacity he is directly responsible for the administration of regulatory compliance and the direction and supervision of the company's over-all accident and injury prevention program.

In nearly 18 years of transportation experience, Mr. Mayerhofer served the Mid State-Spector Freight Lines as safety director. He also carried on safety supervision work for Gateway Transportation Co. and transportation administration for Hayes Freight Lines. Serving as a commissioned officer in the U.S. Army, he gained additional experience in vehicle operations and safety. He has an engineering background, formal training in traffic and transportation management, is certified as a director of motor fleet safety and is a member of several national safety organizations: the National Safety Council, the ATA Council of Safety Supervisors and the American Society of Safety Engineers. He is a past chairman of the safety section of the Central Motor Truck Association.

Robert P. Shumate

Mr. Shumate was a trooper in the Maine State Police for 6 years, 1949-1955. During this time, in 1953-1954, he attended the Traffic Institute at Northwestern University, where he was a participant in the 9-month training program in traffic police administration. In 1955, he joined the field staff of the Traffic Institute, and, in this capacity, developed and directed a training program for the newly organized Wisconsin State Highway Patrol. He was appointed to the position of Assistant Director of Research and Development at the Traffic Institute in 1957, and was made Associate Director of Research and Development in 1958. He then was Assistant Director for Research and Development of the Field Service Division, International Association of Chiefs of Police, Washington, D. C. In that position, he engaged in directing an expanding program of research in motor vehicle traffic supervision, and the development of research projects in other major areas of police activity. He is currently the Director of Research, Department of Police Administration, at Indiana University.

The Effects Of 0.10 Per Cent Blood Alcohol On Driving Ability

Results of a Practical Experiment Conducted at Austin, Texas on January 4 and 5, 1961

by J. D. Chastain

INTRODUCTION

In 1939, interpretations of chemical tests for alcohol for medicolegal purposes were established and 0.15 per cent was recognized as the level at which all people were considered under the influence.[1] Since that time, various experiments, tests, and investigations have been made concerning the validity of this definition. A 1953 report of the National Safety Council's Committee on Tests for Intoxication recognized that an individual's driving ability is impaired at much less than 0.15 per cent blood alcohol.[2] In December 1958, a symposium on alcohol and road traffic was held at Indiana University attended by many of the leading authorities in this field at which the following recommendation was made: "As a result of the material presented at this symposium, it was the opinion of this committee that a blood alcohol concentration of 0.05 per cent will definitely impair the driving ability of some individuals and, as the blood alcohol concentration increases, a progressivly higher proportion of such individuals are so affected, until at a blood alcohol concentration of 0.10 per cent, all individuals are definitely impaired."[3]

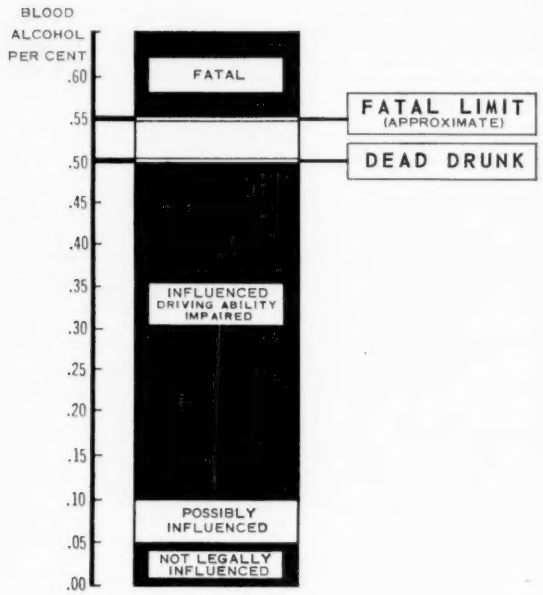
At their 1959 meeting, the Traffic Committee of the International Association of Chiefs of Police passed a resolution supporting the previous statement.[4] In October 1960, the National Safety Council's Committee on Alcohol and Drugs unanimously passed the following recommendation: "That the Committee on Alcohol and Drugs urges the state legislatures when amending or enacting chemical test laws to establish the three blood alcohol levels at 0 to 0.05 per cent, 0.05 per cent to 0.10 per cent and above."[5] In November of 1960, the American Medical Association House of Delegates passed the following recommendation: "That blood alcohol of 0.10 per cent be accepted as prima facie evidence of alcoholic intoxication. Recognizing that many individuals are under the influence in the 0.05 per cent range."[6]

The participants in this experiment have recognized for several years that an individual's driving ability is impaired long before the blood alcohol concentration reaches the previously accepted level of 0.15 per cent. This experiment was conducted at Austin, Texas, on January 4 and 5, 1961, to give the participants an opportunity to observe at first hand how much impairment is produced by lower levels of blood alcohol. Analyses on blood and urine were made by the Houston police department laboratory.

EXPERIMENTAL

Subjects selected as drivers in this experiment were considered average drivers. None were police drivers. On the first day of the experiment, normals were established for each of the six drivers and his eyes were tested using a Titmus Optical Company T/O vision tester.

The driving test as shown in figure 1 was executed as follows: The driver was required to be at 20 m.p.h. when the vehicle reached point A; he would then slow down, make the right turn, then again reach 20 m.p.h. by the



time the vehicle reached point B. He would then start executing the diminishing alley test and had to be at 20 m.p.h. at the time he entered the alley at point C, then go through this diminishing alley with a smooth stop at point D, where the distance from front bumper to the line was measured. He would then go immediately into the off set street manuever forward, at point E would stop and return through the off set street in reverse and stop at point F. Time was recorded for completing the course between points A and F. Then the subject would go forward and be in high gear at 20 m.p.h. at which time the reaction time was calculated by a brake detonator. The drivers were allowed to practice the course until their ability to cover this course was at its maximum. Then the drivers were given four graded runs which were timed between points A and F. On the following day each driver was given sufficient alcohol to get him near the 0.10 per cent range and allowed to drive through the course four times, again graded and timed.

Drivers used the same vehicles, which were equipped with dual brakes, and were graded by the same person with conditions being as close as possible both days except on the second day when they were drinking alcohol. The scorers were training and safety education specialists with many years of experience in this type of grading. The alcoholic influence report form was completed by state highway patrolmen.

Figure 2 shows the type of grading record and error points scored on each driver. On the day of the drinking the following schedule was followed: H, hour started drinking; H plus one hour and thirty minutes, completed drinking; H plus two hours, begin driving test for four complete turns. At the completion of the driving test, breath, blood, and urine specimens were obtained; then immediately the eye test was given and a physical examination made using the National Safety Council's alcoholic influence report form.

DISCUSSION

Figures 3 and 4 show the data that were compiled as a comparison between normal and with alcohol for each driver.

The driving errors as shown in figure 3 are totals of the four runs and reflect the points that are scored against the subject. Zero is a perfect score. It is obvious that all the drivers made more driving errors while taking alcohol than while normal. Subjects 1 and 6 were more affected than the others. For four of the drivers, time for the course was greater while taking alcohol. Two of the subjects drove the course in less time, however they made more driving errors. Reaction time was increased for all six of the drivers while taking alcohol.

Figure 4 shows the data on the vision test. Two of the individuals showed no change in visual acuity while taking alcohol. The other four individuals showed some change with subjects 3 and 6 showing very pronounced changes. Color vision was not affected in any of the subjects. Stereo depth showed some change in four of the individuals with one being a very marked change. Subject 4 had practically no stereo vision, and it did not improve while taking alcohol. Three of the individuals showed slight changes in lateral phoria and one individual showed slight change in vertical phoria. On the visual acuity, subjects 3 and 6 dropped to less than the instrument manufacturer's recommendation for operators of mobile equipment. While they were taking alcohol, the stereo depth of subjects 1 and 6 dropped below the limits recommended by the manufacturer of the instrument. The changes on other subjects were still within the normal limits as recommended for operators of mobile equipment. As stated previously, breath, blood, and urine specimens were taken immediately after the driver completed his driving course while taking alcohol. Only the blood is reported since it was not the intent of this paper to present a correlation although the correlation was very good.

In the physical examination, one subject was graded as "apparently no alcoholic effects," two were graded "slightly" and the remaining three were graded "obviously under the influence."

CONCLUSION

All six of the subjects stated they felt in their own opinions that their driving abilities were definitely impaired to such degree that they would not ordinarily attempt to operate a motor vehicle in that condition.

Each participant took part in the experiment and was able to observe many things and aspects which it is not possible to record by scores. Two very important factors that must be considered in evaluating this experiment are: (1) The number of subjects; (2) the conditions for testing which are never the same as actual driving conditions. Nevertheless, under scrutiny by the participants, it was obvious that all of the subjects' driving ability deteriorated.

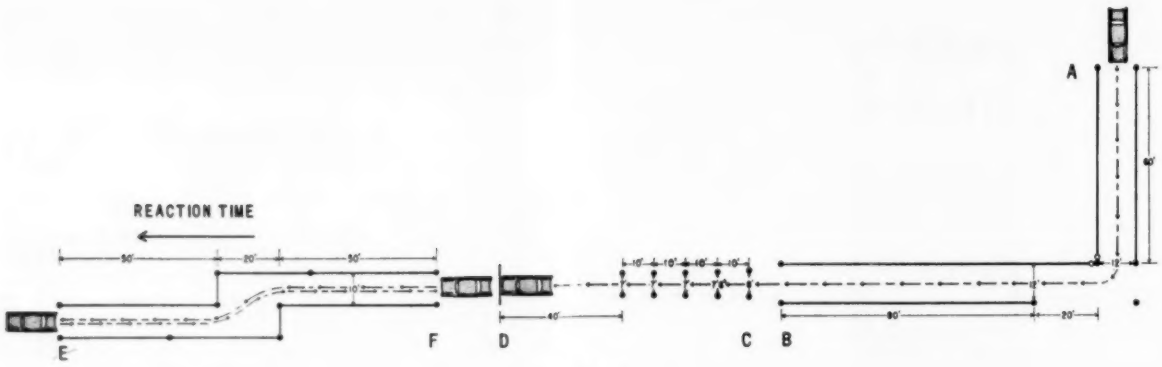


FIGURE I

This small number of test subjects should not be considered as conclusive in itself. However, the data compiled here compared with reports of numerous other researchers and experiments support the conclusion that all individuals are impaired to such extent that they cannot safely operate a motor vehicle when the blood alcohol concentration reaches 0.10 per cent and above.

RIGHT TURN TEST

1. Did not signal on approach. (4) (or improper signal) — — — —
2. Was under 20 m.p.h. at first stanchion. (8) — — — —
3. Touched stanchion or curb. (5) — — — —
4. Ran over stanchion or curb. (10) — — — —
5. Reversed direction to miss stanchion or curb. (4) — — — —
6. Did not use second gear in turn. (3) — — — —
7. Stalled engine. (5) — — — —
8. Was under 20 m.p.h. at last stanchion. (8) — — — —

DIMINISHING ALLEY TEST AND SMOOTH STOP

1. Clashed gears or raced engine. (1) — — — —
2. Reduced speed under 20 m.p.h. (5) — — — —
3. Applied brakes before entering narrow part of alley. (10) — — — —
4. Jerked car when starting or stopping. (2) — — — —
5. Touched stanchion. (2) — — — —
6. Ran over stanchion. (8) — — — —
7. Skidded tires in stop. (5) — — — —
8. Stopped with front bumper short of or beyond line. (For each ½ ft. or fraction) (1) — — — —
9. Stalled engine on stop. (5) — — — —

FIGURE II

OFFSET TEST

1. Stanchions touched (Each 5) — — — —
2. Stanchion run over. (Each 10) — — — —
3. Reversed direction to miss stanchion. (3) — — — —
4. Stalled engine (5) — — — —
5. Door open while car moving. (5) — — — —
6. Raced engine (obvious). (2) — — — —
7. Spun tires on start. (5) — — — —
8. Obviously rough stop. (2) — — — —

Totals: — — — —

TIMES:

EMERGENCY STOP TEST WITH ELECTRIC DETONATOR

Speed of vehicle—20 m.p.h.
Reaction time distance (Feet)
Count fraction of foot. — — — —

	Driving Error Points	Driving Error Point Change	Average Course Time (Seconds)	Average Reaction Time (Seconds)	Blood Alcohol Per cent	Examination Opinion
SUBJECT #1						
Normal	18	——	47.12	.515		
With Alcohol	112	+94	52.25	.667	.12	Obvious
SUBJECT #2						
Normal	26	——	47.87	.516		
With Alcohol	77	+51	51.87	.534	.10	None
SUBJECT #3						
Normal	18	——	62.30	.360		
With Alcohol	64	+46	57.00	.400	.10	Slight
SUBJECT #4						
Normal	12	——	63.17	.327		
With Alcohol	57	+45	56.75	.404	.10	Slight
SUBJECT #5						
Normal	3	——	63.55	.299		
With Alcohol	62	+59	64.20	.402	.11	Obvious
SUBJECT #6						
Normal	42	——	48.37	.540		
With Alcohol	107	+65	57.75	.574	.12	Obvious

FIGURE III

VISION TESTS

	Visual acuity both eyes Snellen equivalents	Stero Depth	Color Vision	PHORIAS Vertical	PHORIAS Lateral
SUBJECT #1					
Normal	20/15	Normal	Normal	Normal	Normal
With Alcohol	20/25	77.7% change	No change	No change	No change
SUBJECT #2					
Normal	20/18	Normal	Normal	Normal	Normal
With Alcohol	20/18	11.1% change	No change	No change	No change
SUBJECT #3					
Normal	20/20	Normal	Normal	Normal	Normal
With Alcohol	20/35	No change	No change	No change	6.6% change
SUBJECT #4					
Normal	20/17	Practically None	Normal	Normal	Normal
With Alcohol	20/17	No change	No change	No change	No change
SUBJECT #5					
Normal	20/17	Normal	Normal	Normal	Normal
With Alcohol	20/20	33.3% change	No change	No change	No change
SUBJECT #6					
Normal		Sub. Normal		Normal	Normal
With Alcohol	20/20 20/50	22.2% change	Normal No change	14.2% change	9.9% change

FIGURE IV

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Beaumont, Texas

McClain, Bill, Chemist, City Health Dept.

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6. Minutes of 1960 Annual Meeting, American Medical Association.

Performance in a Gymkhana Sports Car Event with Low Levels of Blood Alcohol

by Robert B. Forney, Francis W. Hughes, Harold R. Hulpieu and Lt. C. A. Davis

Research on behavior of subjects that have received alcohol usually involves a specific component of behavior—performance. The performance is usually studied with a possible projected correlate to tasks involved in daily occupational and social patterns of man. In an experimental design, it is expedient that blood alcohol levels should closely approximate that which is of interest in human problems. Low blood levels of alcohol were reached in this study purposely because such concentrations best represent a zone of impedence in performance which is difficult to demonstrate. It is not too difficult to demonstrate a diminishment in performance capabilities with levels of blood alcohol that obviously unmask imperfections in personality and enhance deficiencies in locomotor activities.

EXPERIMENTAL PROGRAM

Performance

Driving skill in man was measured in a gymkhana sports car contest. This event was staged by the Indianapolis regional chapter of the Sports Car Club of America. Scoring was computed as outlined by rules of the club. The five driving events selected for the course were designed to demonstrate several types of driving skills (Figure 1).

This paper was prepared by the Department of Pharmacology, University of Indiana, the Indiana State Laboratory for Toxicology and the Indiana State Police. These agencies were aided by the Accident Prevention Program, U. S. Department of Health, Education and Welfare.

A. The first event (Fig. 1-A) was run in reverse gear. The driver backed his car in and out of alternate pylons spaced from 20 to 40 feet apart.

B. The second event (Fig. 1-B) was composed of diminishing right turns. Right turns are difficult to make and by diminishing them, the difficulty is increased. From a standing start the cars were to accelerate 200 feet to the first turn. At this time the cars were making speeds in excess of 40 miles per hour. The first circle was 120 feet in diameter and the last, about 45. The pylons carried numbered flags to keep drivers on the proper turn.

C. The third event (Fig. 1-C) consisted of a maze composed of 109 pylons on a rectangle of concrete 330 feet long and 40 feet wide. The pylons were positioned to offer several possible pathways. The driver, from a standing start, was to pick his way as rapidly as possible through the maze, turn around two pylons 32 feet apart outside the maze and return through it to the finish line.

D. The fourth event (Fig. 1-D) was composed of pylons arranged as three parking garages 20 feet deep and 9 feet wide. From a standing start the contestant accelerated and had to enter completely the first garage 100 feet away. He then reversed into the second garage 100 feet further on and then forward into the third garage 130 feet still further. At this point he was to reverse out and proceed forward into the second garage, reverse into the first and then forward across the finish line.

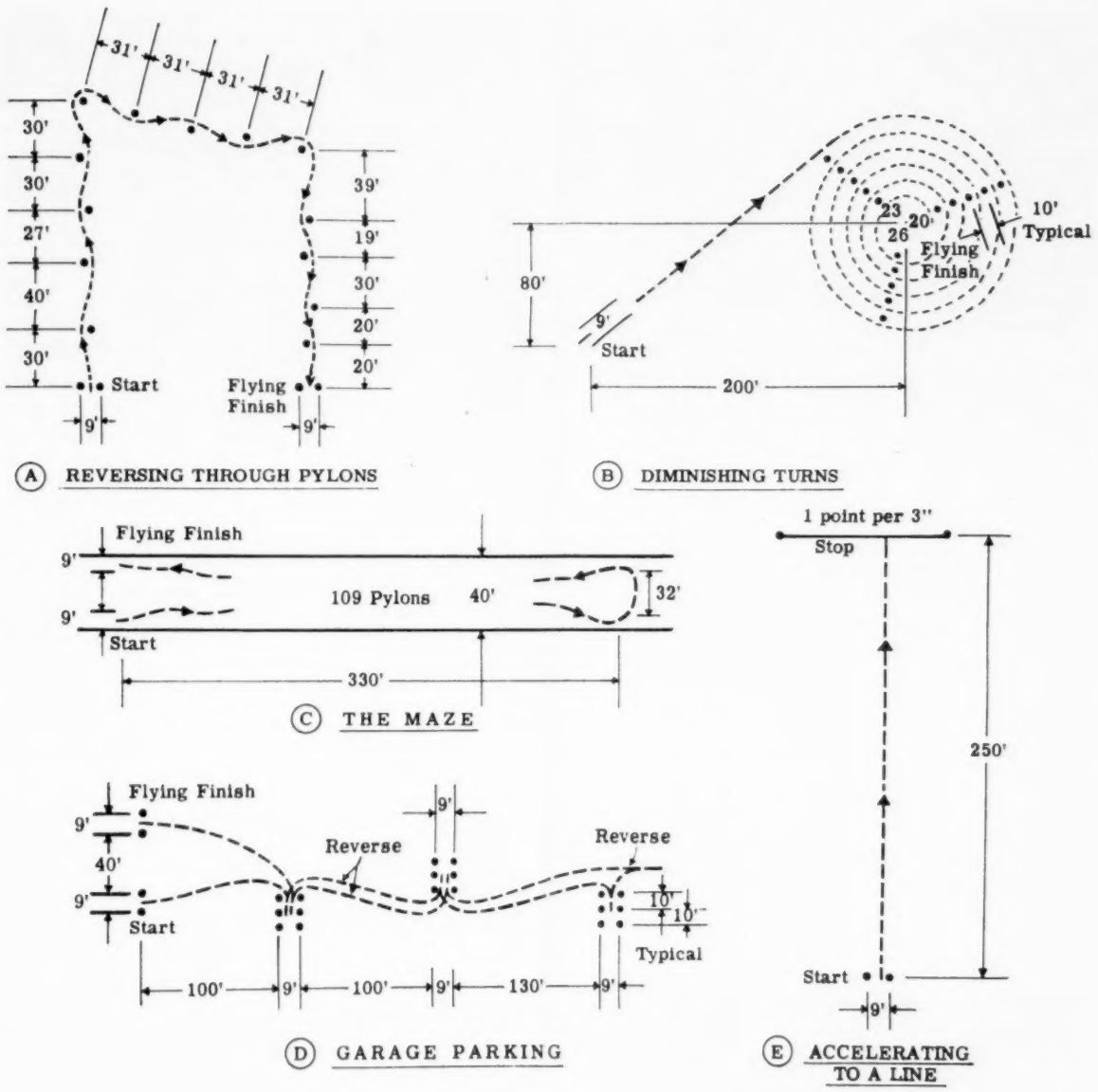


Figure 1

E. The final course (Fig. 1-E) was a straightaway 250 feet long. From a standing start the car accelerated as rapidly as possible and the driver attempted to stop with his left front hub directly over the finish line.

Scoring

Driving skill was determined by totaling the points arbitrarily assigned to the variables measured on each event. One tenth of a point was awarded for each tenth of a second required to complete the event. Three points were added for each pylon hit and ten points for each pylon knocked over. Ten points were added for failure to completely enter the garages in the fourth event. On the last event one point was added for each three inches by which the front left hub missed the finish line. If a driver failed to negotiate a course properly, he was given the highest score earned by any driver on that event during the same trial run.

Chemical Tests

An operations tent was set up convenient to the starting point of the course. Indiana State Police technicians were assigned to analyze the breath of the drivers. Five of them were equipped with Harger Rebreathing Drunkometers and five with Borkenstein Breathalyzers. Registered nurses were employed to obtain blood specimens. Five additional technicians were assigned to make immediate white blood cell and eosinophile counts and to prepare the blood for future analyses for alcohol, cholesterol, sodium, potassium and chloride. Urine specimens were collected to be examined for cystine and protein, and microscopically for sediments.

Subjects

Forty-nine drivers, experienced in gymkhana sports car events, were selected and the general aims of the experiment were outlined to them. The drivers were divided into two groups, balanced as nearly as possible for age, weight and sex. Participants were told that the gymkhana would be typical with awards made to the first five places in each group. Group I was arbitrarily designated the control group and Group II, the alcohol group.

Each group was subdivided into squads of five. An attendant was assigned to each squad to keep them on schedule for the collection of specimens of blood, breath and urine, driving the courses and ingestion of the beverage.

A 12.5 per cent solution of ethyl alcohol in a fruit juice was prepared. The fruit juice consisted of a 50-50 mixture of grapefruit and pineapple juice. To each gallon was added 2 ml. of tabasco sauce which made detection of alcohol difficult without making the juice unpalatable. Each driver received 12 ounces of the fruit juice mix for each 150 pounds of his body weight. The alcohol given the drivers was calculated to raise their blood levels to approximately 0.05 per cent.

Procedure

Breath, blood and urine specimens were collected from each driver and he was fed a standard lunch consisting of two meat sandwiches, fruit jello and a beverage.

The drivers were started through the course at two-minute intervals in the order of their assigned numbers. The first run of the course was designated as control A. As soon as the last driver had completed the first of the five events, driver No. 1 began his second run. Ten minutes after driver No. 1 completed his second control run, his attendant gave him his fruit drink and timed him to consume it within 30 minutes. At two-minute intervals, then, ten minutes after completing their second control run through the course, the drivers in order began their drinks. After the 30-minute drinking interval, each driver was allowed one more hour for absorption of the alcohol into the blood. At this time, a second breath specimen was taken and the drivers began the third run thrugh the course. When this last trial was completed, the drivers returned to the operations tent to submit a third breath specimen, a second blood and a second urine specimen.

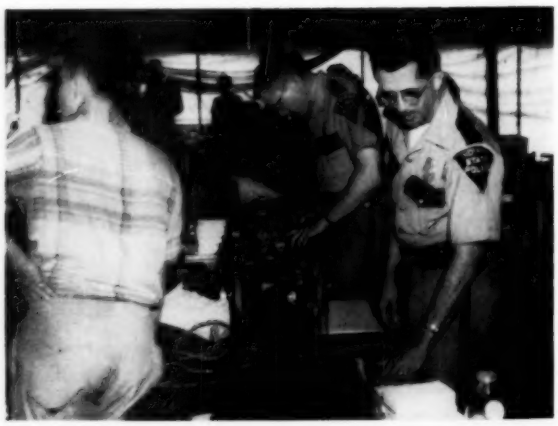
Results

A comparison of the second breath analysis and the third breath analysis indicates that the final trial through the course was made at the peak concentration of alcohol in the blood. The percentage of alcohol in blood based on breath analysis, averaging 0.056 ± 0.003 when the second breath specimen was taken and averaged 0.051 ± 0.002 for the last. The results of direct blood analyses for levels of alcohol in the range of 0.05 per cent were satisfactorily predicted by the Harger Rebreathed Drunkometer and the Borkenstein Breathalyzer operated by competent technicians. Under the conditions of this experiment the correlation of blood and breath alcohol was excellent.

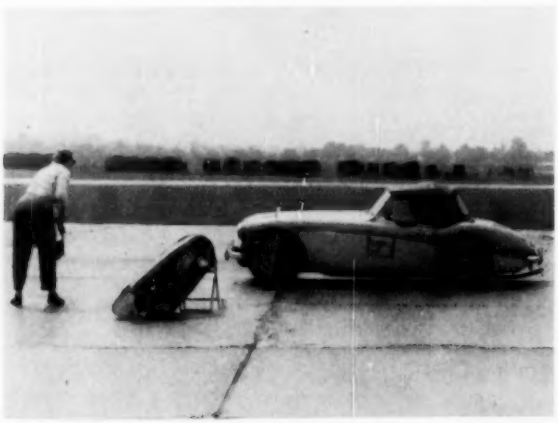
Chemical and microscopic findings are recorded in Table I. In both groups the percentage drop from morning to evening in eosinophile count was approximately the same, viz. 39.3 per cent in Group I and 42.5 per cent in Group II.

In the non-drinking group a rise in white cell count was noted in 15 subjects and a fall in seven (in one subject, sample was lost). In Group II (drinkers) the total white count rose in 17 subjects and a fall was noted in seven. (Two specimens were lost.)

Twenty subjects in the control group (non-drinkers) had increases in serum cholesterol. In one there was no change



Indiana State Policemen prepare testing equipment.



Starter gives Austin-Healey driver the green flag.

Nurse takes blood sample from one of contestants.

Mercedes pilot comes to a halt in front of timers.

and in one the level decreased. In Group II (drinkers), serum cholesterol was determined for 24 of the subjects; there was an increase in 20, no change in three and in one there was a decrease. Serum cholesterol analyses were made on 29 subjects selected at random in the medical school. Blood was drawn at 10:00 a.m. At noon they were fed lunches comparable to those given the drivers in the gymkhana. At 4:00 p.m. a second blood specimen was taken. In the morning the values averaged approximately 209 mgm. per cent. In the evening the average was 210 mgm. per cent.

Table I

Blood Changes

	Group I (Non-drinkers)		Group II (Drinkers)	
	Before	After	Before	After
Eosinophile	121.0	47.5	72.4	30.7
W. B. C.	7,706.0	9,008.0	6,938.0	9,792.0
Serum Cholesterol (mgs. %)	197.0	204.0	201.0	209.0
Na	138.6	141.8	139.2	142.2
Potassium	4.1	3.7	4.0	3.6
Chloride	107.4	106.1	107.5	108.5

In each group serum sodium levels increased in all but one subject. Eighteen subjects of Group I and 19 subjects of Group II showed a decrease in potassium. Eleven subjects of the control group showed an increase in serum chloride.

The changes in the character of the urinary sediment as evaluated microscopically seemed to bear no relation to either ingestion of ethyl alcohol or the time of day the specimens were collected.

Semi-quantitative estimates for cystine and protein in the urine revealed that protein was present in the morning or control urine specimen of one person and cystine was present in the control urine of another. In the afternoon, five urinary specimens had protein and 16, cystine. The changes were unrelated to the presence of alcohol in the blood.

A master scoring sheet was prepared by the chief scorer of the sports car club and is an accumulation of the data supplied by the course judges. The data for the five events was separated into three subdivisions, one for each of the three trials: run A, run B, and run C.

Each of these divisions was subdivided into four columns. The total time in tenths of seconds elapsed during completion of an event was put in the first column, headed "TIME." The second column, headed "OVER," indicates the number of pylons knocked over. In the third column, the number of pylons touched was recorded as "HIT." The fourth column recorded the total score for each event, which was the sum of time plus penalties for pylons hit or knocked over. Awards were made to the first five places in each group to eliminate the influence of alcohol.

Driving skill was determined by totalling the points arbitrarily assigned to the variables measured on each event. One tenth of a point was received for each tenth of a second required to complete the event. Three points were added for each pylon hit and 10 points for each pylon knocked over. Ten points were added for failure to completely enter the garages in event four. On the last event one point was added for each three inches that the front left hub missed the finish line. If a driver failed to negotiate a course properly, he was given the highest score earned by any driver on that event during the same trial run.

A statistical analysis of the data showed no significant difference between the two groups on their control runs. After drinking there was a very significant ($p<0.007$) impairment demonstrated in the drinking group in event one (reverse gear). No statistical evidence of impairment in the drinking group was shown by analysis of data in the other four events.

The statistical analysis of the data treated the contestants as two groups, the drinkers and non-drinkers. Individual performances under the conditions of the experiment were,

therefore, hidden and the excellence of many drivers' scores could mask the poor showing of a few.

It was possible that some drivers could have been handicapped with alcohol and still would show a general improvement with practice in the events. This improvement might not have been as great as would have been obtained in the absence of alcohol. No measure of such effects was apparent from the data collected.

While it is generally agreed that a blood alcohol of approximately 0.05 per cent will not produce measurable deterioration of motor skills or judgment in all people, it is also generally conceded that some persons may exhibit measurable deteriorative effects. It might be argued that, with the low levels of alcohol produced in this study, some of the drivers receiving alcohol would improve their scores more than would have been expected on the basis of practice alone.

In studying the records of single drivers, we thought it possible to determine the presence of impairment or improvement in a few as a result of the alcohol they had ingested.

The difference scores (time plus penalties) made by both groups in all their trials and on all five events were examined for the incidence of positive differences. A positive difference would result only when a higher score was made on the second of the two trials compared.

The sums of the events on which positive differences occurred were recorded numerically for each driver. A decrease in the number of events on which higher scores were made would indicate improvement. Conversely, an increase could suggest impairment.

Among the drinkers, drivers No. 4 and No. 30 made poorer scores on four events after alcohol than they earned on their second control run. Drivers No. 6, No. 35 and No. 46 made higher scores on all five events after alcohol.

The average increase in scores in these events for these five drivers was *15.2*. An examination of the control runs made by these drivers revealed that each earned a higher score in just one event on the second trial. Evidence of impairment is thus indicated in these five drivers in the drinking group. The data supports the observations made on the day of the experiment that these drivers were under the influence of the alcohol ingested.

None of the drivers among the non-drinking group made a higher score on more than three events during his third trial compared to his second. While seven drivers of this group did compile higher scores on three events, the average increase per event for each contestant was only *3.4*.

Five drivers of the drinking group made distinctly poorer scores on the last trial than they compiled on the second control run. These scores were significantly higher than those earned by the seven poorest performers of the non-drinking group.

The results of this preliminary study suggest that driving impairment is produced in some people with low levels of blood alcohol.

THE EFFECT OF PNEUMATIC ROAD TUBES ON VEHICLE SPEEDS

by Richard F. Crowther, Robert P. Shumate and R. Dean Smith

THE PROBLEM

For many years, "before and after" speed studies using various mechanical or electronic devices such as pneumatic road tubes, "radar," and others, have been used to evaluate the effect of many changes relating to street and highway designs as well as the effect of legal requirements on driving speed.

During recent years there has been increased speculation concerning the relationship between speed and accidents, leading, in part, to a more critical interest in the relationship between enforcement effort and speed reduction on high accident roadways and in the general behavior of vehicles as demonstrated by their measured speeds. The problems in this area of interest are in theory well suited to experimental study, utilizing "before and after" study methods. In actual practice there are several perplexing problems that have tended to hamper experimental study in this field.

Perhaps the paramount problem in connection with speed measurements is the development of suitable sampling methods which are economical and which provide a high level of reliability in detecting real changes in speed behavior as

opposed to those that result from sampling error. Methods of sampling commonly employed today do not on the whole satisfy this criteria and when critically examined are effective only in measuring gross changes in speed behavior produced by an introduced variable.

A vital part of the problem of sampling design is related to the effect that the measuring device itself has upon the true or "real speed" of vehicles being measured. "Real speeds" are the vehicle velocities that are influenced only by the normal conditions present. "Measured speeds" may be real speeds. That is, the speeds measured at a point on the roadway may reflect the true velocities of vehicles passing that point and not be influenced by the measuring device, as for example when the device is concealed. On the other hand, "measured speeds" may not be "real." The speeds indicated by the instrument will reflect the true velocity at a point but the magnitude of the velocity may have been influenced by the presence of the measuring instrument itself. The vehicle operator may react to seeing the measuring device (road tubes) and take his foot off the accelerator or even apply his brakes. Thus the "real speed," the speed at which the vehicle would have been traveling, is affected by the device and is, therefore, not "real" but merely "measured."

The question "Are measured speeds the same as real speeds?" is an extremely important question that must be answered in connection with sampling design. At present there are two general types of speed-measuring devices used for making speed studies. Devices operating on the radar principle make up one group and are of such a nature that with simple modification the instrument may be completely concealed from the driver whose speed behavior is being measured. This feature eliminates the problem of the measuring unit itself affecting driver speeds, but this advantage is somewhat offset by the fact that automatic recording of speeds is expensive, requires bulky and complex equipment, and lacks accuracy when less complex and expensive equipment is used.

The second group of speed-measuring instruments, while more readily adaptable to automatic speed recording, utilize pneumatic tubes stretched across the highway as triggering devices to measure speed over a preset (and usually short) distance. The fact that the tubes used as triggering mechanisms are visible to drivers and that such instruments are frequently used for enforcement work has led to considerable speculation about the effect that such tubes themselves have on a driver's speed.

This study is directed specifically toward an evaluation of the utility of speed-measuring devices that use pneumatic tubes. The essential question examined is whether speeds measured by pneumatic road tubes are true speeds or whether the tubes themselves produce a material bias in "measured speeds." The remaining sections of this report are devoted to an examination of these questions. The body of the report contains a description of the sampling model, the statistical design, the field work, and data interpretation. Generally speaking, the mathematical concepts used in the statistical design are contained in a later section of the report and only essential mathematical and statistical concepts are discussed in the text.

EXPERIMENTAL DESIGN

Definitions

For consistency and ease of reference the following terms, as defined, will be used throughout this report.

Real Speed is used to mean the speed of a driver influenced only by random variable conditions that are part of the normal driving environment.

Measured Speed refers to the velocity of a given vehicle at a single point in time and space as measured by some type of measuring instrument.

Control Sample is a sample consisting of 15 matched pairs of measured speeds, one having been taken at a point designated A and the other at a point designated B, while speeds are affected only by random variable conditions.

Test Sample is a sample consisting of 30 matched pairs of measured speed taken in the same manner as those in the control sample with the exception that the variable to be examined is in place at location B.

Test Set is a group of samples consisting of a test sample preceded and followed by a control sample.

Tube Configuration is a specific combination of tube color, spacing, highway design, surface coloration, and legal speed limit used as controlled variables in a test sample.

Basic Assumptions

As in any experimental design certain basic assumptions were made prior to the start of the experiment. The principal assumptions that underly this study are:

1. An observable difference exists between real and measured speeds when pneumatic road tubes are placed upon the highway during the time and at the place the tubes are positioned.

2. The extent to which real and measured speeds differ will be influenced by the ability of the driver to perceive the tube configuration and react by changing his speed behavior.

3. The extent of driver reaction and the resultant difference between real and measured speeds will depend upon the recency and extent of road tube use, in the home area of the driver, for enforcement purposes.

4. The difference between real and measured speeds will be negative; i.e., measured speeds will be some quantity less than the real speeds.

5. Variability of an individual driver's real speed at two specified points on a given highway is influenced only by random variable conditions, the effect of which is a constant at both positions.

6. The speed of groups of drivers selected from different time periods of the day will show significant difference even under normal conditions with only random variable influences at work.

These assumptions permit the development of a more formal model to test the relationship between real and measured speeds. We may examine a specific set of causal factors with the end to quantifying their effect on measured speeds.

Test Procedures

A series of highway locations were selected which met predetermined specifications: concealed speed-measuring devices were placed so that the speed of any specified vehicle could be measured at two separate points on the highway. Traffic traveling in only one direction was observed. The first point at which speed measurements were made will be referred to as station "A" and the second, station "B." Thus, a vehicle entering the test site, going in the direction selected for study, must first pass station "A" then station "B." The distance separating the two measuring positions was dependent upon the sight distance confronting the driver at position "A." The criterion for determining distance was that a driver at "A" was unable to see either road tubes or equipment in place at "B."

The measured speed at point "A" and point "B" was recorded on a data sheet in matched pairs, for example:

	Speed in mph	
	A	B
Observation 1	51	52
Observation 2	47	46
Observation 3	50	53
Observation 4	62	62

In the example the matched pair of speed measurements in observation one (51 and 52), indicates that a vehicle passed points "A" and "B" at 51 and 52 mph, respectively. This was the general form of all samples taken. Each observation consisted of two measured speeds—one at point "A" and one at point "B."

All experimental conditions were introduced at point "B" and the speeds at point "A" were influenced, under assumption five, only by random variable conditions existing at the site during the time the samples were taken. The further assumption was made that during both the control and experimental sampling the random variables present would affect the measured speeds at both points at a constant.

The actual sampling of vehicle speeds was carried out as follows: all equipment used for the measurement of vehicle speed was concealed from the view of passing drivers throughout both the test and experimental samples. First a control sample consisting of 15 matched pairs of speeds was taken with all equipment to be used in the test sample completely removed from the scene. When the control sample was complete, the controlled variable (pneumatic tubes) was placed on the roadway at point "B," and an experimental sample consisting of 30 matched pairs of speed observations was taken. At the completion of the experimental sample the controlled variables was removed and a second control sample of 15 matched observations was taken. Each sampling set was taken in the same manner with a different configuration of tube and roadway being used for each set. This method permitted statistical comparison of the "real speeds" of vehicles passing points "A" and "B" with speeds at the same points after the introduction of a variable.

General Theory

The basic technique used in this study is a modification of a study method usually referred to as "Before and After." Vehicle speeds are measured at a point on the highway with conditions as nearly normal as possible. After the initial sample is taken a variable is introduced and a subsequent sample taken at the same spot. Differences in the speeds between the two sets of data are assigned to the effect of the introduced variable. The design of this experiment, however, differs considerably from the traditional "Before and After" study technique.

In the design used here we started with two measurements of the same driver's speed at two points on the highway designated as point "A" and point "B." If all drivers drove at a constant speed regardless of highway design, trip purposes, or other external causes, the problem would be a relatively simple one. In such a case, a driver's speed measured at point "A" would always be an absolute predictor of his speed at point "B." Unfortunately, this is not the case. The speed of individual drivers varies from point to point on the highway and the speed of groups of drivers tends to vary from time to time throughout the day.

Even though such variability exists, speed measured at point "A" may be used as a predictor for expected speed at point "B" if certain conditions are met. If a constant statistical relationship between points "A" and "B" exists, then "A" may be used as a predictor of speed at "B," assuming that the defined relationship between "A" and "B" remains constant throughout the period of time involved in the study.

Once again the nature of driver behavior is such that this latter condition is not completely satisfied. While a statistical relationship between "A" and "B" may be adequately specified for a limited period in time the relationship will vary from time to time throughout the day. Since this variability throughout time is not, on the basis of current knowledge, predictable, safeguards must be established in both the experimental design and interpretive methods to permit its detection.

In this study we were concerned with measuring the effect that a particular configuration of road tubes had upon the speed of drivers. We assumed that the effect in general would be negative; that is, show a decrease in speed. In order to isolate the effect of the road tubes from the normal variability of individual driver speed it was necessary first to establish the statistical relationship between real speeds at points "A" and "B" when influenced only by random variable conditions. As was described earlier this was done by measuring the speed of 15 vehicles at both points with all test equipment concealed and the variable to be tested removed. Utilizing the variability of speeds between "A" and "B" the relationship may be defined such that for any measured speed at "A" we may predict the same driver's speed at "B." There are two formal ways of establishing this relationship both of which are discussed in detail in a later section.

When the relationship of driver speeds at "A" and "B" had been established from data collected in the control sample it was possible to introduce a tube configuration at point "B" and again examine the "A" to "B" relationship. With the desired tube configuration in place, a test sample of 30 vehicles was taken, speeds being measured at both "A" and "B." From this data the statistical relationship between the two points could be determined.

We now have determined two statistical relationships that can be used to isolate and measure the difference in speed between the control and test sample. By utilizing appropriate statistical tests described elsewhere in this report, we may determine whether the "A" to "B" speed relationship was significantly different when the tube configuration was in place. We may also examine both the characteristics and magnitude of speed differences at specified points throughout the range of speeds observed.

Such a method is valid only if the statistical relationship between "A" and "B" remains constant throughout the entire test set. To insure that this condition was met, the statistical relationship between speeds at "A" and "B," determined by the control samples, taken immediately before and after the test sample, was compared directly.

If the statistical relationship between the two control samples is the same, we accept the difference (if any) between the statistical relationship in the control sample and that in the test sample as assignable to tube effect. In the event that the relationship between the two control samples is different, we must treat the results as inconclusive.

Variables in the Design

The Roadway

One of the variables which may be assumed to have an influence upon the manner and extent of driver reaction to pneumatic road tubes is the type of roadway on which they appear. There are numerous differences in both design characteristics and surface coloration that may reasonably be expected to affect the driver's ability to see the road tubes on the highway. Macadam and concrete surfaces present a wide range of background coloration which, when combined with prevailing light, might be expected to produce visibility conditions that materially affect the driver's reaction. Our entire hypothesis is based upon the assumption that drivers must first see the tubes before they will react to them. The attentiveness of the driver is another factor closely related to design characteristics of the highway. Highways of advanced design might be expected to produce less attentiveness on the part of the driver to details of the roadway surface directly in his path, thus reducing the likelihood of his detecting the road tubes and reacting to them.

As a means of examining these sources of roadway variations three common roadway configurations were selected which provided for the measurement of variation in reactive behavior which might be related to roadway type. These were:

1. Four-lane, divided, concrete roadway.
2. Two-lane concrete roadway.
3. Two-lane macadam roadway.

Speed

Another variable which may influence stimulus—reaction to visual road tube patterns—is that of mean, roadway speed. The mean speed at which vehicles travel upon roadways varies with the type, design and legal speed limit for those roadways. Vehicle operators on high-speed roads may react differently from operators on roadways with lower speeds. Two speed variables were selected in order that differences in reaction related to speed could be measured. These variables in speed were introduced into the roadway configuration variables as follows:

1. Two-lane concrete, 65 mph limit.
2. Two-lane concrete, 50 mph limit.
3. Two-lane macadam, 65 mph limit.
4. Two-lane macadam, 50 mph limit.
5. Four-lane, divided, concrete, 65 mph limit.

Tube Color

Just as the road surface, in its color or ability to reflect light, may influence the degree of visibility and, therefore, the degree of reaction, so may the color and visibility of the road tubes. There are two common road-tube colors in use at present. These are black and gray. Road-tube color is introduced as a variable in conjunction with the tube width, which is described in the following paragraphs.

Tube Width

Road tubes, when used as speed-measuring devices, are placed on a roadway at specific distances from one another. The more common distances between road tubes, regardless of color, are 88 feet, 66 feet, 30 feet, and 3 feet.

The distance between the tubes placed on the roadway varies with the particular measuring instrument in use. There is such an extreme difference between the several tube widths it was considered essential that each of these distances be tested for differences in reaction, on the part of the vehicle operator, to them.

Combining tube color variability (black and gray) with tube width the following set of conditions was developed:

88 feet black	88 feet gray
66 feet black	66 feet gray
30 feet black	30 feet gray
3 feet black	3 feet gray

Equipment Display

The use of pneumatic road-tube devices in the measurement of speed requires a certain amount of equipment and at least one observer. This equipment and the observer may either be well concealed (except for the tubes, of course) or left in open view of the drivers on the roadway under study. The concealment or display of equipment and observer is considered as a variable in this study since in either case the visual pattern presented to the motorist will change relative to the display or concealment of such equipment and observer.

Tests of this variable were conducted with the two extreme tube widths. The 88 foot and 3 foot widths, both

black and gray tubes, were tested. The final set of variables tested is listed below. Each roadway configuration was tested under all listed tube color and width conditions and under all conditions of concealment listed.

Roadway Configuration	Tube Display and Concealment
Two-lane concrete, 65 mph	88′ black, concealed
Two-lane concrete, 50 mph	88′ black, open display
Two-lane macadam, 65 mph	66′ black, concealed
Two-lane macadam, 50 mph	30′ black, concealed
Four-lane, divided, concrete, 65 mph	3′ black, concealed
	3′ black, open display
	88′ gray, concealed
	88′ gray, open display
	66′ gray, concealed
	30′ gray, concealed
	3′ gray, concealed
	3′ gray, open display

Selected cases of the above conditions were tested during darkness. Some special variables were tested such as the display of single road tubes. These are special cases and are treated separately in the report.

Constants in the Design

Weather, Road, and Light Conditions

All observations, except for special cases, were made during daylight hours. In all cases the roadway was dry. Weather conditions varied from cloudy and bright to clear and sunny. Observations were not made during "dull-overcast" or "stormy" days.

Free Flow Vehicles

All speeds observed were of "free flow" vehicles. A "free flow" vehicle is defined as any vehicle traveling on the roadway which has freedom of movement in a forward direction for not less than 500 feet.[1]

Unusual Events

In the event a "free flow" vehicle was observed and some unusual event occurred at or near the test site, such as overtaking and passing in either direction, another vehicle stopping, leaving or entering the roadway, at or near the test site, or unusual equipment, such as large, slow-moving trucks, farm implements, etc., appeared, the observation was not counted.

Emergency Vehicles or Marked Police Vehicles

When an emergency or marked police vehicle passed the test site going in the same direction as the traffic being studied, the observations immediately preceding or following the appearance of such vehicle were not counted. In the event an emergency or marked police vehicle passed the test site going in the direction opposite from the traffic being studied, no observations were made for a period of five minutes.

Vehicles Sampled

Only four-wheeled, passenger vehicles were observed in this study. No vehicles with trailer attachment of any kind were included in the observations.

Control Samples

The function of the control samples taken in this study are three-fold:[2]

1. Control samples function to measure any change in the uncontrolled variables which may appear at the site during any phase of the sampling.

2. Control samples provide a standard against which any changes observed in experimental data may be measured.

3. Control samples provide a means for examining the underlying assumptions of the experimental model.

Control samples were taken before and after every experimental sample. For example, work was started at Site 1 by removing all visible equipment from view and sampling the speeds at points "A" and "B."

Fifteen matched pairs of speeds were observed, using the concealed measuring instruments at points "A" and "B." The speeds measured under control conditions were considered "real" speeds since they were influenced only by the normal roadway and driver conditions (random variables) present in all samples.

After the initial control sample was taken an experimental condition was introduced at point "B" (road tubes placed on the roadway). After 30 matched pairs of speeds were observed under these conditions all visible equipment was again removed from the roadway and a second control sample taken. This sequence of control, experiment, control, etc., was carried on for all sites.

Because all experimental conditions could not be tested during a single day a control sample was taken at the start and finish of each day's work at a particular site. This resulted in having two control samples in sequence at several places in the collection of data. This is essential since systematic day-to-day and hour-to-hour variation in the population may be present.[3] Such variation can be accounted for best by always starting and ending with a control sample and by always preceding and following all experimental tests with control samples.

Randomized Starting Points

There may be a kind of "learning response" to the presence of experimental equipment on a particular roadway. If such an assumption is true, and one test site was used two or more days in sequence, some of the motorists using the roadway regularly would become used to the presence of the tubes and, therefore, their response to the test situation would deteriorate. To minimize this possibility the field schedule was designed to permit the maximum time lapse between visits to each particular test site.

A further safeguard was introduced on the assumption that even if no test site was visited two days in a row there still may be some effect from the "learning response" during the last sampling periods at each site. If this assumption is true, and the sampling was carried on in the order listed in the sampling schedule (page 14) then the greatest effect of the learning response would be present in the last experimental sample taken at each site, in all cases the 3-foot, gray, road tube condition.

To avoid this, the starting point for each test site was selected at random. After selecting the starting point the sampling was carried on in the order listed. For example, at Site 3 the starting point was condition number 10 (30′ gray). The conditions tested in order after starting at 10 were 11, 12, 1, 2, etc., back around the schedule to condition 9 at which point the work was complete.

Statistical Factors Affecting the Design Section

Until enough work has been done in a field to determine workable and effective experimental designs, each new experiment is, to some extent, a gamble. A design that is excellent under one set of circumstances may be next to useless under others. The particular design adopted in this case represented a compromise among several alternatives. After the choice was made we learned much about the nature of the data that the problem generates, and if the experiment were to be repeated or extended, a better design could be made.

Primarily we wished to determine whether road tubes could be used to collect useful speed data. If, in general, they are unreliable, we wished to know which tube configuration was best. We also wished to estimate the effect and interaction of the tube-environment conditions if possible.

One of the major considerations affecting the design was the difficulty of assigning individual cars at random to test or control situations. The tube and equipment take some time to set up and, even though we recognized the risks involved, we decided to use systematic sampling with blocks of cars assigned to test or control situations.

From previous experience we anticipated that the average speeds of cars passing a given fixed point on the highway would change from hour to hour and day to day, even in the absence of test equipment. We wished to remove this source of variability from the analysis of results. Randomizing the selection of vehicles to be measured would accomplish this, but even if this were possible, the two-measurements-per-car method would still have been adopted. The speed of a car at "A" can be used to predict its probable speed at "B" quite accurately. By using two measurements we eliminated the variability of speeds between cars from the analysis. We had, instead, the variability of the speed of a single car at two points, which was much smaller. Thus, substantially fewer observations were needed with the two-observation-per-car model, and the effect of the systematic variation controlled through measurement.

The control, test, and control sequence were chosen because we felt that the uncontrolled factors affecting speed would be closely associated with time. If such factors operated, they would therefore make the two bracketing controls differ. The number of rejections we obtained from testing the controls was not excessive, given the levels of probability at which we worked. Thus, such factors did not seem to be pertinent within the time interval required to collect the data. There was some evidence that over longer periods of time than that required for a control, test, and control cycle, such factors did operate. The set of all controls at a given location were heterogeneous.

There were two considerations behind our decision to restrict sampling to "free-flow" cars. This restriction was one which is frequently observed in traffic studies and, to that extent, our measures were more relevant. In addition, we hoped that the restriction to "free-flow" vehicles would minimize interaction between driver behavior. To the extent that drivers react to such things as brake lights and changes in speed of preceding cars, the observations in test situations become interdependent or serially correlated. For statistical reasons it was extremely desirable to avoid this type of relationship in data, if it was possible. It should be noted that the reactions to tube configurations may be substantially greater if the speeds of all cars are measured.

The test was designed to provide comparisons of a number of factors which were systematically varied. However, because of the presence of heterogeneity in the results, it is unlikely that any useful result would arise from additional analysis.

In retrospect we have decided that a sequential design would have served us better. In this case a few sets of observations taken in the same general pattern as our present test would probably have indicated that refined measurement of the effects of the various controlled variables was not going to be feasible. At that time a decision could have been made to restrict the experiment to the general questions of road tube effects, or the experiment could have been redesigned in an attempt to provide refined measures. In the latter case, a randomized block design, with replications of each tube configuration and fewer control observations, would be worth trying. The assignment of the blocks should, of course, be completely randomized.

DATA COLLECTION, FIELD METHOD

Instrument Reliability

All speed measurements collected for this study were made with "Electromatic Radar Speed Meters." These radar units were concealed in two standard rural mail boxes placed adjacent to the roadway under observation. The power supply was concealed in the base of the mail boxes and the speed meter attached to a 500-foot cable, leading from the sensing unit in the box to a place of concealment in a farm yard or other private land. The observers were housed in a station wagon parked well back from the roadway and concealed from view of approaching vehicle operators.

At approximately two-hour intervals both radar units were calibrated by exciting the sensing units with a standard, calibrating tuning fork. Adjustment was rarely necessary and then only by a factor of one-half to one-quarter

mile. At the beginning and at the termination of each day's sampling the sensing units were validated by driving a vehicle through the test site at a constant speed. The speeds indicated at each unit as the vehicle passed through were recorded on the data sheet. Any variation between the readings at the meters would result in all data collected being discarded. When, for any reason, the power was shut off at either unit, the calibration and validation procedure would be carried out before resuming the observations.

any given roadway. This point must be predictable. The observer reading speeds at unit "B" will record the last-clear-reading received as each selected vehicles passes through the cone. Being able to predict the point on the roadway at which this last reading occurs permits the observation of speed at point "B" to be made just before, or during the time, the selected vehicle is between the tubes displayed.

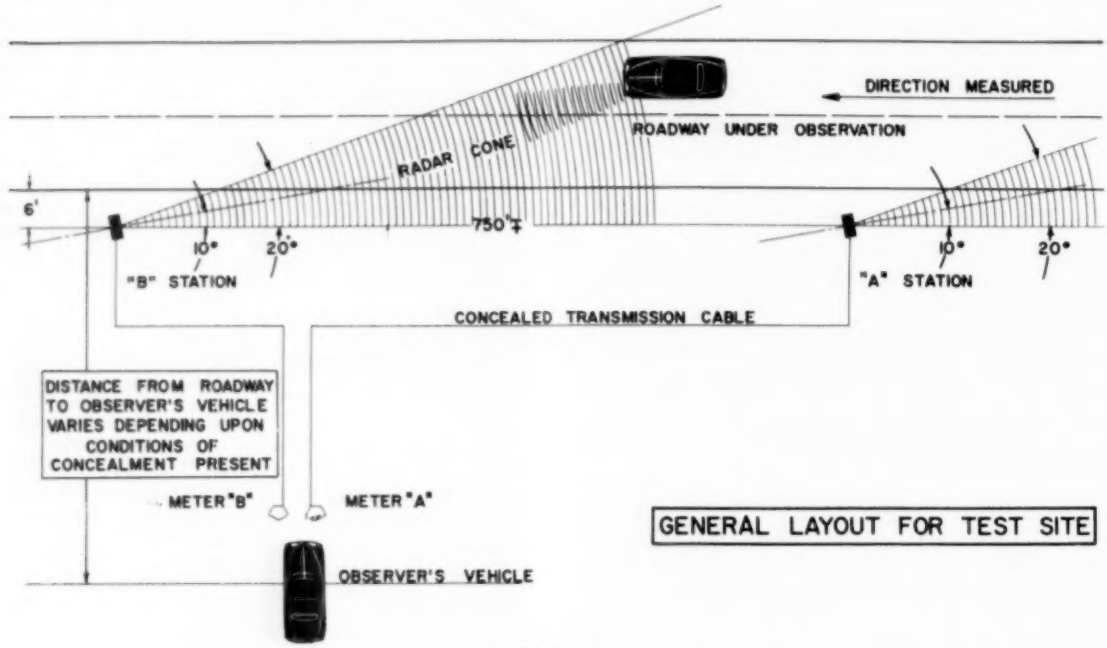


EXHIBIT 1

Tube and Instrument Positioning

Exhibit 1 demonstrates the general arrangement of the instrument in relation to the roadway at any given test site. Exhibit 2 shows the precise relationship between the measuring instrument at point "B," the roadway, and between the instrument at point "B" and the tube layout. The dimensions specified in Exhibit 2, which locate the different tube patterns on the roadway for the experimental conditions, are constant for all sites and all experimental conditions tested.

The necessity for keeping the relationship between tubes and measuring instrument at a constant at point "B" is supported by the following.

The specifications of the sensing unit used (consistently) at point "B" are as follows. The emission beam transmitted from the unit describes a cone 20° from the source outward for a distance of approximately 175 feet (see Exhibit 1). Placing the sensing unit "B" at a constant distance from the edge of the roadway under observation and directing the unit toward traffic at a constant angle relative to the edge of the roadway under observation makes it possible to predict the point of last-clear-reading at unit "B" on

Making Observations

Observations were controlled by the observer reading meter "A." As traffic approached unit "A" the observer determined whether a particular vehicle would qualify as free flow. If it did, he took a reading as it passed sensing unit "A" and recorded this reading on his data sheet (Exhibit 3). He then instructed the observer at sensing unit "B" to "read" that selected vehicle, identifying it as it approached "B." This reading was also recorded on the data sheet. Either observer may have discarded a pair of readings for any reason, such as an unusual event, unsure reading, or loss of the observation at either point on the roadway owing to conflicting vehicular movement.

Concealment of Equipment

In several series of tests one of the variables examined was the effect of equipment normally used in conjunction with pneumatic road tubes. The equipment referred to included air switches, connecting wires, and the recording instrument itself. In those tests where the effect of tubes only was to be studied, all related equipment was completely removed from the vicinity of the highway and the

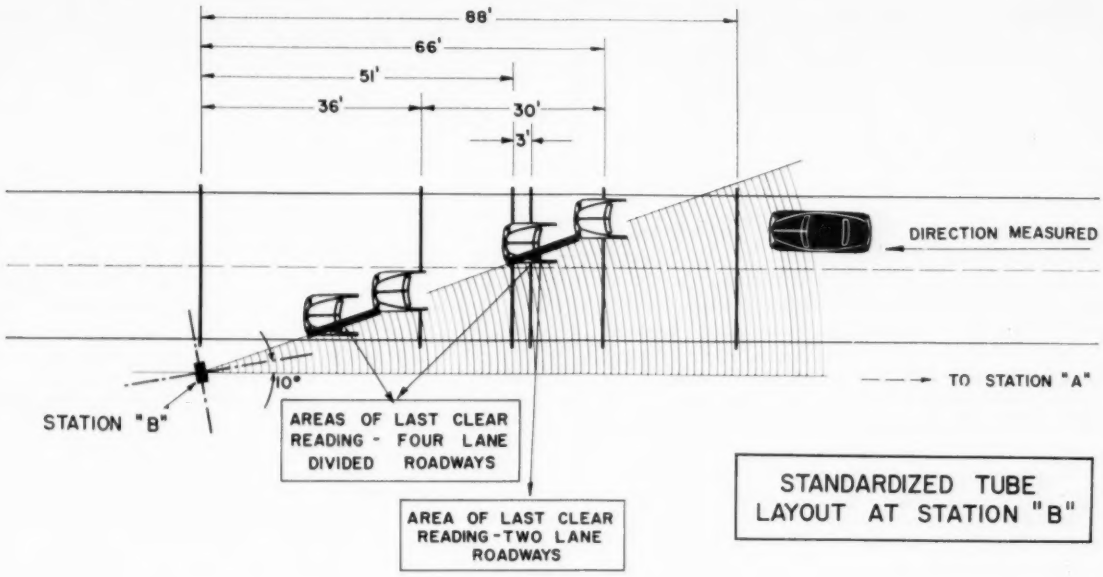


EXHIBIT 2

NO	A	B		
1			LOCATION	SITE NO.
2			TEST CLASS	
3			DIRECTION MEASURED	
4			MONTH · DATE · YEAR	
5			DAY OF WEEK	
6			TIME PERIOD FROM	TO
7			OBSERVER'S NAME	
8			WEATHER	
9				
10				
11				
12			HIGHWAY	
13				
14				
15				
16			INSTRUMENT VALIDATION	
17			INSTRUMENT TEST A B	
18				
19			A	
20			B	
21			REMARKS	
22				
23				
24				
25				
26				
27				
28				
29				
30				
31				
32				
33				
34				
35				

EXHIBIT 3

observers were concealed from the view of passing traffic. The tubes were placed on the highway, but were not connected to any of the normal instruments.

In the tests where the effect of the auxiliary equipment was to be studied, all connections and switches were positioned as they would be for normal operations. No attempt was made to conceal these devices through artificial camouflage. The observers were seated in a car some 20 feet from the roadway with wires leading from the tubes to their position plainly visible.

STATISTICAL DESIGN

The matter of statistical design always poses a difficult problem in presentation. To the reader unfamiliar with statistical methods and reasoning, too detailed a discussion with the attendant mathematical rigor tends to leave him hopelessly confused. On the other hand, to the reader vitally interested in details of the statistical design, anything less than a complete and detailed presentation of the formal arguments involved is unsatisfactory. This section is a compromise between the two extremes. It presents an outline of the methods used and provides a basis for understanding the general techniques by the non-mathematical reader. For the mathematically inclined reader interested in the more exact formulations, a complete discussion of the statistical design is contained in Appendix A. (ed. note: see first footnote)

The statistical design in this experiment evolves basically from the problem of variability in driver speeds between two or more specific locations on a given highway segment. Even in the absence of any introduced variables, it is not possible to choose two points such that the measured speed of each car is the same at both points. However, since the driver and the vehicle are the same, the two speeds tend

to be closely related. For example, if a car passes point "A" at 45 miles per hour, there is a fairly high probability that it will pass point "B" at some speed between 44 and 46 miles per hour, and a very small probability that it will pass point "B" at more than 60 miles per hour. In other words, the speed of a car at "A" may be used to make a prediction of its speed at "B."

Now, two questions arise about the effects of pneumatic road tubes:

1. Does a particular configuration of road tubes significantly bias measured speeds?

2. To what extent (in absolute terms) does a particular configuration of road tubes influence measured speeds?

The first question is statistically simpler than the second. In addition, if road tubes have no measurable effects, the second question is unlikely to be worth considering. Hence, the problem of determining whether road tubes have any effect on measured speeds will be considered first.

Determining the Existence of Tube Effects

For each configuration of road tubes examined we have 30 pairs of speed measurements made with the equipment in place. These are referred to as test data. In addition, there are 15 pairs of speed measurements made before and 15 pairs made after the test data with no equipment visible. These latter are referred to as the control data. These 60 pairs of measurements constitute the data for a single test set.

The speed observed at "A" is subtracted from the speed observed at "B" for each car. The resulting differences are the changes in speed of each car in the interval from "A" to "B." Thus, a difference of —5 indicates a decrease in speed of 5 miles per hour, a difference of 0 indicates that the car speed did not change measurably, and a difference of +5 indicates an increase in speed of 5 miles per hour in the interval "A" to "B."

If the configuration of road tubes tends to cause drivers to reduce speed in the interval "A" to "B," then the 30 test data differences will tend to be lower than the 30 control data differences. Since many factors, other than the presence or absence of speed-measuring equipment, cause changes in measured speeds, the two sets of differences will not be distinct or separate. They will, in general, overlap unless the reaction to the road tube configuration is strong enough to override all other sources of speed variability.

In order to analyze the data, a decision rule or process must be adopted to determine whether the set of test differences is in some sense smaller than the set of control differences. In the selection of these rules we must recognize that the differences are subject to disturbances by unmeasured causes. The decision rule must recognize that part of the differences between the sets is not attributable to road tubes but to other chance elements.

A fairly simple set of rules which uses a minimum of assumptions is available. The 60 differences are put in order of size and then ranked. That is, each difference is assigned some number between 1 and 60 inclusive. The bigger the difference the larger is the rank assigned to it. In those cases where there are ties, where more than one car decreased speed by the same amount, the same average rank is assigned to each member of the tie. This is done in such a way that the sum of the 60 assigned ranks is equal to the sum of the whole numbers 1, 2 . . . 60. The sum of the first N whole numbers is given by the formula

$$\frac{N(N+1)}{2}$$

In this case for $N = 60$,

$$1 + 2 \ldots + 59 + 60 = \frac{60(60+1)}{2} = 1830.$$

If road tubes cause drivers, in general, to reduce speed the difference in the test data will tend to be smaller than those in the control data. The ranks assigned to the test sample will tend to be smaller than those assigned to the control sample. In particular, the sum of ranks assigned to the control set will exceed the sum of ranks assigned to the test sample, and the sum of control sample ranks minus the sum of test sample ranks will be positive. This difference in the sum of ranks is essentially the statistic which is used to determine whether the control set differences exceed materially the test set differences. From our point of view this is equivalent to deciding whether or not a particular road tube configuration affects driver speeds.

The difference between the sums of ranks assigned to the test and control sample by construction must be a whole number from the set —900, —899 . . . —1, 0, 1, . . . 899, 900. Assume that road tubes do not affect speeds, and that the control and test sample differences are the same except for randomly operating disturbances. Then the probability of any particular one from the 1831 numbers —900, —899 . . . —1, 0, 1, . . . 899, 900 arising may be computed. Hence, by adding the probabilities attached to —900, —899, . . . we may find a number T such that the probability of a difference of T or less arising is greater than 0.95. Then if the null hypothesis is true (tubes have no effect), the probability of observing a difference in sums of ranks of T + 1, T + 2 . . . 899, 900 is less than 0.05. T in this case may be referred to as a "critical value" of the statistic. This critical value divides the set of all possible values of our test statistic into two regions. If the null hypothesis is true (tubes have no effect on speed), then the statistic will lie in the lower region about 95 per cent of the time, and in the upper region about 5 per cent of the time. If, however, the null hypothesis is false and the alternate hypothesis is true (tubes cause reduced speed), the probability of observing a value in the lower region is reduced and the probability of observing a value in the upper region is correspondingly increased. We will decide that the tube configuration has no measurable effect on car speeds if the value of the statistic is less than or equal to T. If the statistic is greater than T, we will decide that road tubes do affect car speeds.

Using these rules we will wrongly decide that tubes do affect speed when in fact they do not about 5 per cent of the time. The probability of this error occurring would be reduced to say 0.01 by using a new critical value R which divides the set of possible values of the statistic into 99 per cent and 1 per cent regions. This reduction in the probability of deciding in error that tubes affect speed must be paid for by an increase in the probability of deciding in error that road tubes do not affect speeds when in fact they do.

As an example let us consider the decision process for the case when there are four observations in the test sam-

ple and four observations in the control sample. For simplicity, let us assume there are no ties in the data. Then each observation will be assigned a rank, some whole number between 1 and 8. The difference between the sum of ranks assigned to the test and control samples must be one of the set of numbers 0, ±2, ±4 . . . ±14, ±16. (Odd numbers arise only if we permit ties between members of the test and control sets.)

Exhibit 4 gives the probability of observing a difference in the sums of ranks of —16, —14, and so on for each possible difference. These probabilities are derived on the assumption that the test and control sets are random samples from the same population.

EXHIBIT 4

THE PROBABILITY AND CUMULATIVE PROBABILITY OF SPECIFIED DIFFERENCES IN SUMS OF ASSIGNED RANKS*

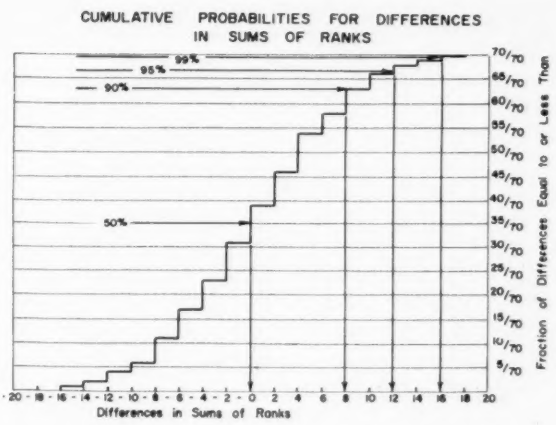
Difference in Sums of Assigned Ranks "Control" minus "Test"	Probability of each Difference	Cumulative Probability of each Difference or Less
—16	1/70	1/70
—14	1/70	2/70
—12	2/70	4/70
—10	3/70	7/70
— 8	5/70	12/70
— 6	5/70	17/70
— 4	7/70	24/70
— 2	7/70	31/70
0	8/70	39/70
2	7/70	46/70
4	7/70	53/70
6	5/70	58/70
8	5/70	63/70
10	3/70	66/70
12	2/70	68/70
14	1/70	68/70
16	1/70	70/70

*Values for all possible differences for two random samples of size 4 from the same population, assuming no tied ranks.

In addition, Exhibit 4 shows for each possible number the probability of a difference of exactly that amount or less arising. The probability for —14 is the probability of —14 arising plus the probability of —16 arising. For —12 this probability is the probability of —12 arising plus the probability of —14 arising plus the probability of —16 arising. The other cumulative probabilities are constructed in a similar way. It should be noted that the probability of a difference of +16 or less arising is 1.

Exhibit 5 shows graphically the same information given in the last column of Exhibit 4. The left hand vertical scale is in decimal instead of ratio terms. From this chart it is possible to derive the critical value for any level of probability. For instance, the 50 per cent line intersects the probability line at a difference of 0. Thus, we know the probability of a difference in the sum of assigned ranks of 0 or less arising is greater than or equal to 0.50. Similarly the critical value, which divides the set of possible differences into 90 per cent and 10 per cent regions, can be found to be +8 from the intersection of the 90 per cent

EXHIBIT 5



line with the probability curve. The chart shows there is no critical value which divides the set of possible differences into 99 per cent and 1 per cent regions. For under the assumptions, with these small samples, all the test set ranks could be smaller than all the control set ranks more than 1 per cent of the time by chance alone. With a larger sample the problem does not arise. Consider two hypothetical cases:

Case 1 Differences		*Case 2* Differences	
Test	*Control*	*Test*	*Control*
—3	10	9	12
7	2	4	10
5	—1	5	11
4	6	3	8

For each case list the difference in order of magnitude and assign ranks. (The differences from the control sets and the corresponding assigned ranks are in parentheses.)

Case 1		*Case 2*	
Difference	*Rank*	*Difference*	*Rank*
(10)	(8)	(12)	(8)
7	7	(11)	(7)
(6)	(6)	(10)	(6)
5	5	9	5
4	4	(8)	(4)
(2)	(3)	5	3
(—1)	(2)	4	2
—3	1	3	1
Ranks Test	*Assigned Control*	*Ranks Test*	*Assigned Control*
7	8	5	8
5	6	3	7
4	3	2	6
1	2	1	4
—	—	—	—
17	19	11	25

Sum of Ranks Assigned

From this, the statistic, the sum of ranks assigned controls minus the sum of ranks assigned tests, can be computed in each case.

Differences in the sum of assigned ranks control minus test:

Case 1: 19 —17 = 2 Case 2: 25 —11 = 14

For a 95 per cent test the critical value of the difference is 12 (as illustrated in Exhibit 5). Thus, for Case 1, since 2 is less than or equal to the critical value of 12, we accept the hypothesis that the two samples come from the same population. In this study such a result would be interpreted as implying that the particular tube configuration does not affect speed.

In case 2, 14 is greater than the critical value 12. We would, therefore, reject the hypothesis that the test and control samples came from the same population. We would adopt the decision that the test sample differences are smaller than the control sample differences. This is equivalent, in this study, to accepting the conclusion that the particular road tube configuration did affect speeds.

This example is given primarily to demonstrate the logic of the decision rules which were chosen. The computation actually used differs in a number of details. Since speeds are recorded to the nearest mile per hour there are several ties in each control and test set. In order to avoid the lengthy computations which arise in determining critical values for such cases the statistic has been modified so the relevant distribution is in approximately standard "normal" form. Critical values can be found in a standard statistical table. The same statistical test is used on each pair of control samples which bracket a test sample. This is done to give some protection against the dangers arising from the lack of randomization.

Size of Speed Differences Attributed to Road Tubes

In the preceding section we discussed a test designed to answer the question "Do road tubes have some effect on measured car speeds." Since the test utilized ranks rather than data in miles per hour it contributes little information to questions about the magnitude of the effect of road tubes on car speeds. It is possible for such a test to find highly significant results in cases where there are systematic changes so small as to be of no practical importance. From an operational point of view, we need some information about the magnitude of the effects of road tubes in some relevant measure such as miles per hour. This additional information can be gained only at the cost of more restrictive assumptions and in this case more complex statistical logic and computations.

The method which was selected involves the construction of explicit predictors of the speed of a car at "B" as a function of its speed at "A." Two such predictors are constructed for each road tube configuration, one based on the control data, the other on test data. If, in fact, the road tubes do cause reduced speeds then the speeds predicted at "B" by the test data predictor will tend to be lower than the corresponding speeds predicted by the control data predictor. The difference between the predicted speeds at "B" is an estimate in miles per hour of the effect of that particular road tube configuration.

The construction of explicit predictors also opens the questions of correction factors or functions. To what extent is it possible to utilize road tubes in speed data collection, and then "adjust" observation where necessary to allow for the road tube effect? The use of predictors yields some information about the role of such factors as speed limits and tube-road surface contrast which have been varied systematically in the experimental design.

The specific form of the predictor which was adopted is $Y = aX + b$ where Y is the natural logarithm of the predicted speed of the car at "B" and Y is the natural logarithm of the car speed observed at "B." The term X is the natural logarithm of the observed speed of the car at "A," while a and b are constants determined by the least square techniques on the observed data sets.

Under some conditions refined comparisons are possible. In general, detailed comparisons have been made only among simple test samples and the two control samples which bracket it.

By constructing a predictor from the test data and another from the related control data, estimates can be derived for the speed with which a car traveling at a given speed at location "A" will pass location "B," under both the test and control conditions. The difference in these speeds is the estimated magnitude of the effect of the tube configuration on measured speeds.

The problem of whether or not these differences in predicted speeds are the result of chance fluctuations or the systematically introduced tube configurations also must be considered. This problem can be formulated in statistical terms as the question "Do the test data and control data come from populations having the same exponential regression line?" It should be noted that this does not require the test and control data to come from populations which have the same average. Thus, it is possible for the average speed of cars passing through the test site to change over time without the statistical test becoming invalid for that reason alone. As long as the relationship between the speed at "A" and the speed at "B" remains unchanged the speeds of the cars entering the test area, in general, do not matter.

Here again some decision rule must be adopted. The risks are of the same kind as those in the ranks test decision rule. However, the analysis is more complicated, and only an outline of the argument will be presented here.

For any observation the difference between the predicted speed at "B" and observed speed at "B" is a residual error. The distribution of the sums of the squares of the residual errors is known, by assumption. This distribution is the same for the residual errors about a single predictor based on the pooled control and test data if the control and test data come from populations having the same regression line. If the test and control data come from the populations having materially different regression lines, a predictor based on the pooled test and sample data will fit poorly. This poor predictive power will be reflected by a relatively large sum of squared residual errors. An "F" test can be established which essentially compares the magnitude of the squared residual errors for the predictor based on the test data and control data separately, with the magnitude of the squared residual errors for the predictor based on

the combined test and control data. The results of this test, the "F"-test, and their interpretations are described in the section "Interpretation of Results."

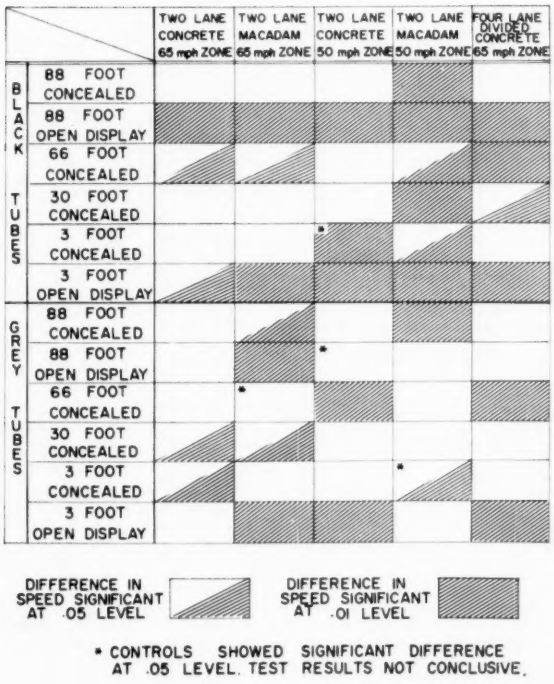
INTERPRETATION OF RESULTS

The series of tests conducted during this study resulted in 64 configurations of tube, highway, and display sets being evaluated. Subsequently we will discuss the apparent effect of specific combinations of spacing, coloration, and highway characteristics. However, as to the general question "Do pneumatic tubes give biased results in measuring speed?" the answer seems to be an emphatic "yes," regardless of type.

Exhibit 6 shows the 60 combinations of circumstances tested and the results achieved. Of the 60 tests made, at the 5 per cent level, 31 showed a significant difference between the expected or real speed when tubes were in place. If chance alone were operating to produce the changes observed, we would normally expect to have three of the 60 show a significant difference in the true speed. The probability of observing as many as 31 significant differences in 60 test sets by chance alone is considerably in excess of one time in a thousand. It also should be noted that all of the significant differences are negative; that is they show significant *decreases* in the expected speed rather than increases.

EXHIBIT 6

TESTS SHOWING SIGNIFICANCE USING THE MANN-WHITNEY U TEST


TWO LANE CONCRETE 65 mph ZONE
TWO LANE MACADAM 65 mph ZONE
TWO LANE CONCRETE 50 mph ZONE
TWO LANE MACADAM 50 mph ZONE
FOUR LANE DIVIDED CONCRETE 65 mph ZONE
BLACK TUBES
88 FOOT CONCEALED
88 FOOT OPEN DISPLAY
66 FOOT CONCEALED
30 FOOT CONCEALED
3 FOOT CONCEALED
3 FOOT OPEN DISPLAY
GREY TUBES
88 FOOT CONCEALED
88 FOOT OPEN DISPLAY
66 FOOT CONCEALED
30 FOOT CONCEALED
3 FOOT CONCEALED
3 FOOT OPEN DISPLAY
DIFFERENCE IN SPEED SIGNIFICANT AT .05 LEVEL
DIFFERENCE IN SPEED SIGNIFICANT AT .01 LEVEL

* CONTROLS SHOWED SIGNIFICANT DIFFERENCE AT .05 LEVEL. TEST RESULTS NOT CONCLUSIVE.

An examination of the normal deviates given in Table 1 in Appendix B shows that of the 60 tests only two show increases in expected speed values with tubes in place, neither of which approaches significance. The other 58 show decreases in expected speed. This in itself is highly significant since if the variation in speed were attributable to chance only we would expect to find roughly 30 of the tests showing increases with the remaining 30 showing decreases. The likelihood of observing such a bias by chance considerably exceeds the .001 level of probability. A further examination of Table 1 reveals that of the 29 tests that did not show significance at the .05 level, the majority tend to approach this level quite closely.

With few exceptions, there do not seem to be any strong indications that specific combinations of tubes and highway design produce either better or poorer results. This is unfortunate, since such results virtually rule out the possibility of deriving general correction factors for tube effect. One general exception is that grey tubes seem to produce slightly less reaction than black, regardless of the pavement coloration on which they appear. However, the advantage enjoyed by grey tubes is apparently not sufficient to make them reliable measuring devices, with 12 of the 30 tests with grey tubes showing significant decreases in the expected speed. The concealment of equipment such as wires, air switches, and the operator of the equipment tends to produce some improvement in the recorded speeds, but again it is not enough to make results reliable.[4] Referring again to Exhibit 6, 16 of the 24 tests conducted with equipment openly displayed showed significant changes while only six out of 24 showed significant decreases when equipment was concealed.

An examination of these tables reveals that in a remarkably large number of cases the expected difference between real and measured speed becomes more pronounced at the upper speed ranges. This characteristic has serious implications for the research worker who plans to use speed measurements taken with pneumatic tubes to study upper percentile ranges. The results reported here indicate that speeds in the upper percentile ranges in most cases are most seriously affected.

While these results say little about the amount of difference that occurs in miles per hour, they are extremely important from a research standpoint. The worker who wishes to study the effect of an introduced variable on speed behavior must rely upon some statistical method involving probabilities to determine when observed changes are real and may be assigned to the variable being studied. The fact that the effect of tubes alone will produce statistically significant differences between real and measured speeds will invariably produce results that are misleading.

An important question regarding the effect of pneumatic tubes on vehicle speeds is, "Assuming tubes do produce speed readings significantly lower than true speed, what are the magnitudes of the decreases?" The question as posed is a complex one with many ramifications. Because we are dealing with a problem which essentially involves driver perception and reaction to certain stimuli we must recognize that reactive behavior as well as perception will change in relationship to the velocity with which the driver is traveling. This implies that the magnitude of the difference between real and measured speed is not a constant

throughout the range of speed that may be observed at a given location. We may speculate that at least two factors contribute to this variability of reactive behavior throughout the speed range.

1. The actual rate of forward movement at which the driver is traveling limits both the time available to observe the stimulus and to react prior to entering the zone of measurement.

2. The stimulus itself (pneumatic road tube) has symbolic value to the driver principally as it is related to the legal speed limit within which he must travel at the specified point on the highway.

It is important from the standpoint of research studies that the worker have some knowledge of the actual amount of difference between real and measured speed likely to be encountered at specified speed ranges. To answer this question the differences between real and measured speeds have been calculated for all 64 test configurations and are presented in Tables II through XXXIII in Appendix B. The data presented represent the differences at 5 mph intervals between the control regression estimates at point "B" and the test regression estimates at the same point. The logical development of this test is discussed in detail under the section entitled "Statistical Design."

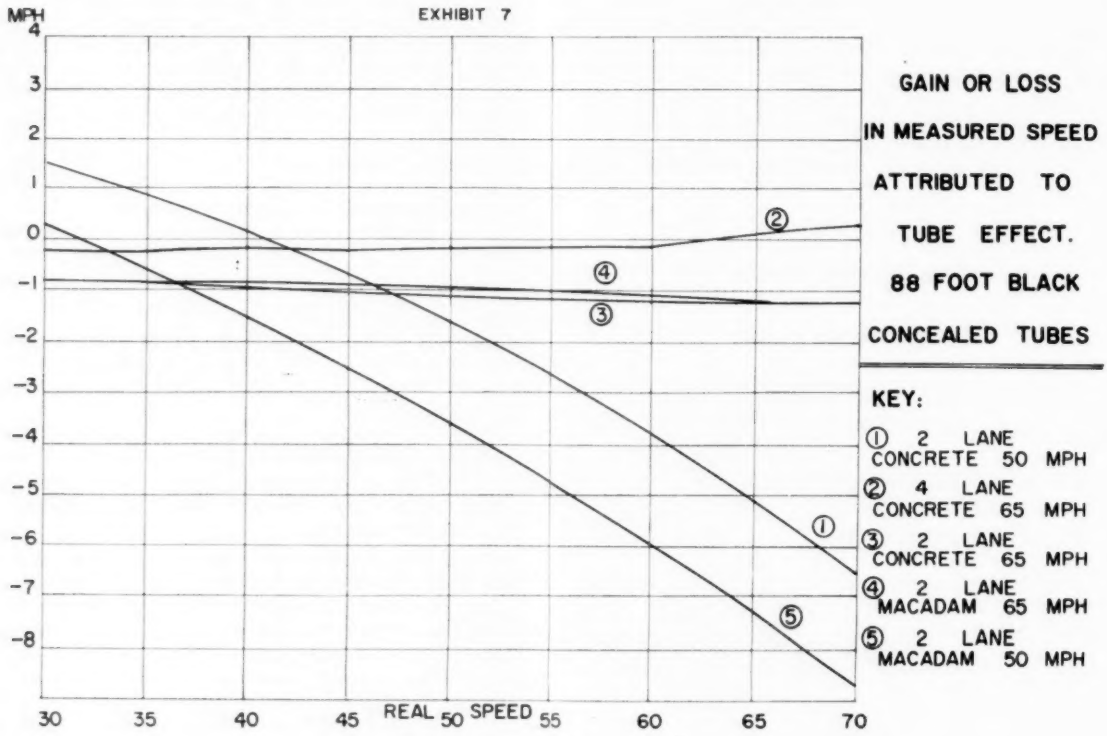
The behavior of the control samples deserves comment at this point. Referring to Exhibit 6 we find that in five cases of the 60 test sets presented, the controls bracketing the test samples showed significance at the .05 level. Normally we would expect exactly three controls to show significant difference by chance and the probability of obtaining five significant differences is quite high.

The behavior of the control samples when compared with that of the test samples adds even more weight to the conclusion that tubes do materially bias measured speeds.

Variability in the speed of individual drivers between two points on the highway seems to be predictable when only random variable conditions are operating, but the effect of pneumatic tubes is to consistently produce measured speeds at the test point which are lower than those predicted.

The wide range of factors examined poses a problem in presenting significant results with clarity. To facilitate presentation the remainder of this section will be divided into a series of subsections, each discussing one aspect of the effect of specific tube-highway-speed configurations on speed values at selected points.

EXHIBIT 7



Tube Spacing

The distance between tubes does apparently produce different patterns of reactive behavior. Exhibits 7 and 8 illustrate this effect, using the two extremes in spacing—88 feet

EXHIBIT 8

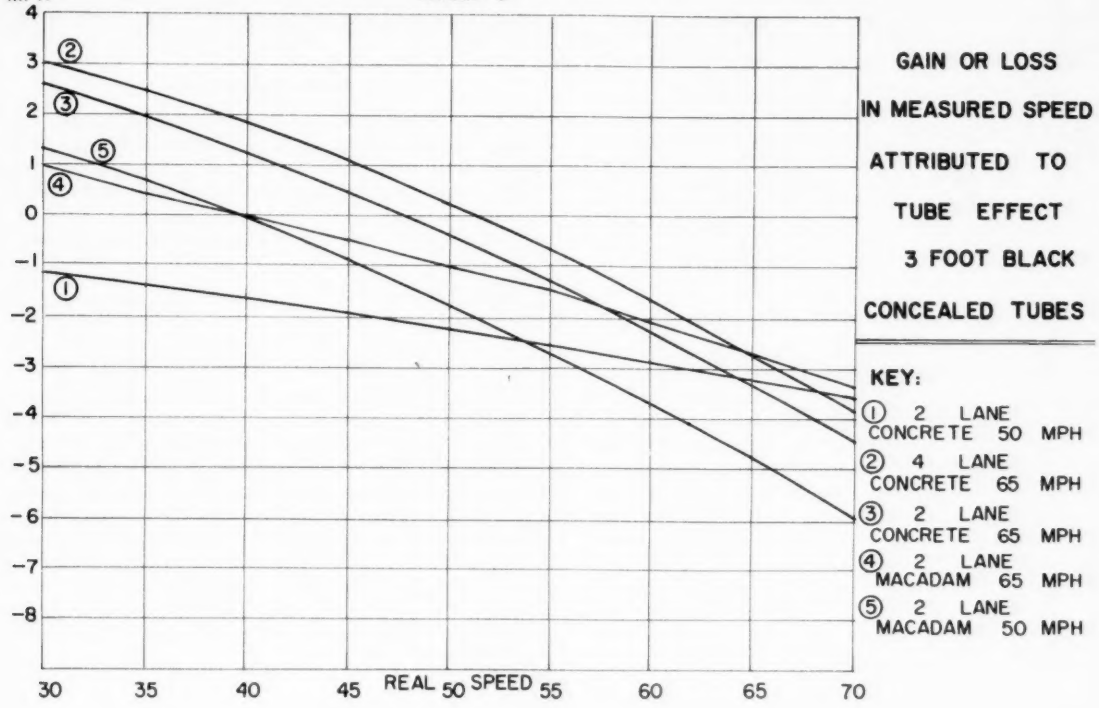
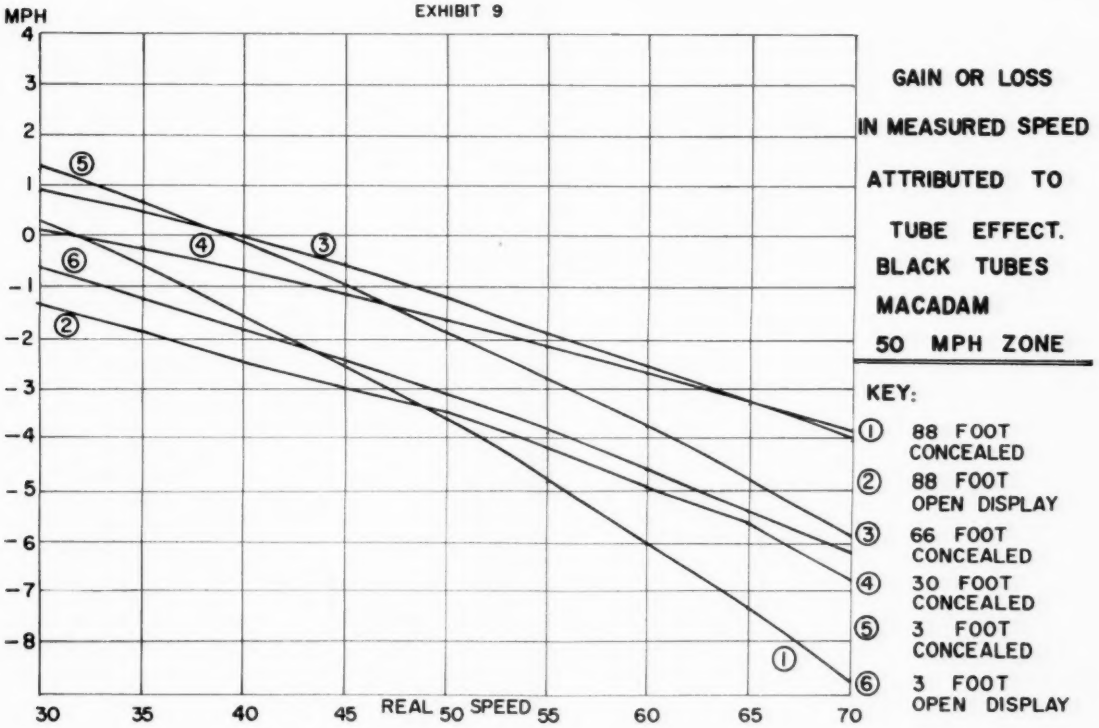


EXHIBIT 9



and 3 feet. Wide tube spacing, as shown in the exhibits, produces little difference between real and measured speed throughout the range when legal speed limits and average speed are high. On the other hand reaction is sharp when legal limits and average speeds are lower. In contrast, as tube spacing is reduced the difference between real and measured speeds becomes well defined with the magnitude of the difference increasing sharply as actual speeds go up. While only the two extremes in spacing are illustrated, this pattern seems to be consistent throughout the test sets with the differences between real and measured speed tending to become more sharply defined as tube spacing is decreased.

Tube and Highway Coloration

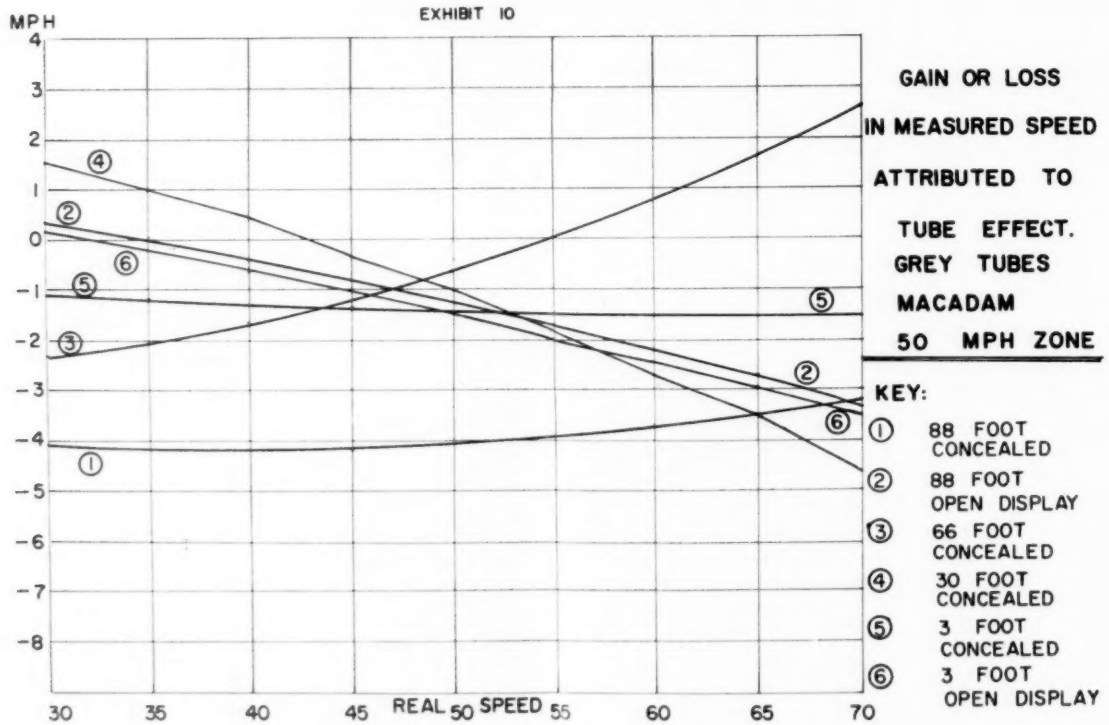
The effect of color contrast is not as clear as that of spacing. On the whole it appears that black tubes produce more pronounced reaction than grey tubes regardless of the highway surface color on which they appear. Exhibits 9 and 10 are typical of the differences encountered. Both show expected differences in speed on macadam surfaced highway in a 50 mph zone. The black tubes show a consistent and rather sharp difference between real and measured speed while grey tubes follow a less clearly defined pattern which seems to be true in general regardless of the surface coloration or speed zone with which they are used. Highway surface coloration, independent of tube color, seems to have little consistent effect with the possible exception that black tubes on concrete at 66 feet spacing and above seem to produce less reaction.

Legal Speed Limit

Legal speed limits and average speed seem to be factors primarily when tubes are widely spaced. Returning again to Exhibits 7 and 8, the typical pattern appears. When tubes are widely spaced (66 feet or more) reaction is slight when speed limits are high. Conversely reaction is sharp when limits are lower, 50 mph in this case. Where tube spacing is close as shown in Exhibit 7 reaction is well defined at all speed levels.

Instrument Concealment

Whether the instruments used to measure speeds in connection with pneumatic tubes are concealed or left in plain view of drivers seems to have little effect when tube spacing is close as illustrated by comparing Exhibits 8 and 11. However, when spacing is lengthened the effect of equipment displayed seems to be considerable, although an examination of the curves in Exhibit 12 fails to show any high degree of consistency. Both two-lane macadam and concrete highways with speed limits of 65 mph show a different response to that of other groups. It is difficult to assign any logical cause for this difference, although in general it would seem

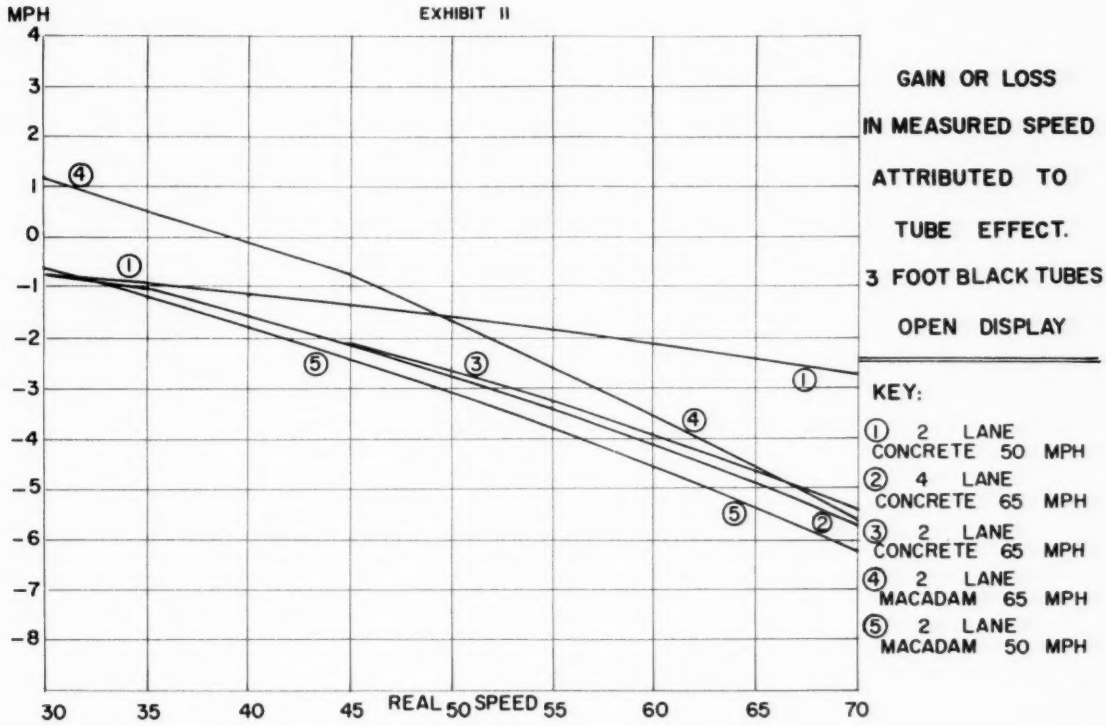


that instrument concealment or lack of it is relatively unimportant when compared with the effect that the road tubes themselves have.

Special Tests

Two special tests were conducted to evaluate the effect of darkness on reaction to tubes and the effect that a single tube has on real speeds. Both tests were conducted on a two-lane macadam highway within a 50-mile zone. Grey tubes spaced at an 88-foot interval showed a significant decrease in expected speed with a maximum expected difference of —3.6 miles at 70 mph. Single tubes showed no significant differences in expected speed values.

EXHIBIT II



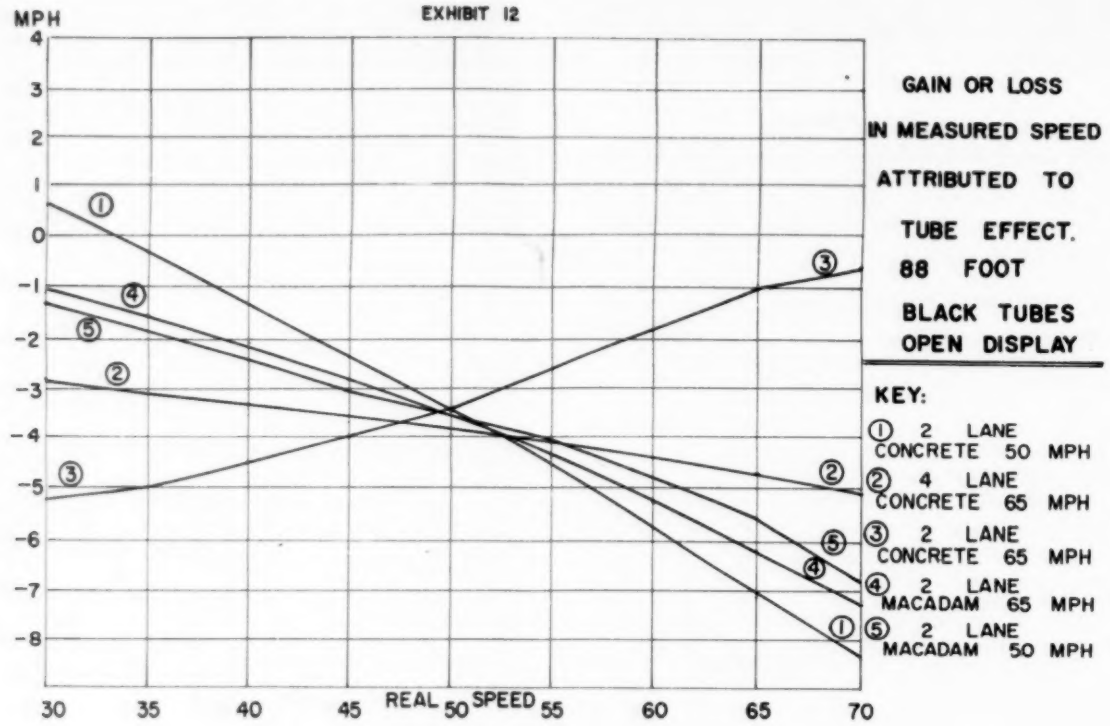
CONCLUSIONS

A review of the results achieved in this series of tests leads to the inevitable conclusion that there are transient variables at work which have not been adequately isolated. We can only speculate as to their nature, but it seems likely that such things as sun position and resultant light and shadow patterns, as well as the trip purpose of the majority of the drivers at any given time period, will exercise considerable influence on the perception and resultant reaction to various tube configurations.

A further condition that would seem to be pertinent is the extent to which pneumatic tube speed-measuring devices have been used in the area for enforcement. There does not seem to be anything inherent in tubes *per se* to cause drivers to react by reducing their speed. This is borne out by the result of the single-tube tests. Reaction is apparently based upon the extent to which the majority of drivers in a given area have learned to regard tubes as a threat of some unpleasant result. As pointed out earlier, pneumatic tube devices had been used extensively for enforcement purposes in the areas in which the study was conducted. In areas where tubes had not been used so extensively, the result may be somewhat different.

These facts, by no means, detract from the validity of the conclusions presented here. The differences reported are real and material and must be considered in making a decision to use tubes to measure speeds for research purposes. From a practical standpoint the existence of undefined transient variables makes it impossible to develop correction factors to compensate for the bias produced by the tubes. The conclusion is that if pneumatic tubes are used the chances are great that significant differences in speed will be observed as a result of tube effect.

EXHIBIT 12



Specifically the conclusions that may be drawn from this are:

1. Pneumatic tubes produce a significant bias in measured speeds as opposed to real speeds, with measured speeds being lower than real speeds.

2. The magnitude of the error in measured speeds tends to increase as real speeds become higher, with the largest errors appearing at the 70 percentile level and above.

3. The error in measured speeds is not systematic for any tube highway configuration and cannot be compensated for by means of a correction constant.

4. Variables that affect the magnitude of error in measured speeds are:

a. Tube spacing
b. Average "real" speed of vehicles
c. Legal speed limits
d. Tube color

5. Certain combinations of tube spacing, color, and speed limits tend to reduce the error in measured speed, but no combination is sufficiently free from error such that it can be consistently relied upon.

One further question that may arise in connection with the use of road tubes is (if tubes are used under identical conditions, except for time): "Will the bias be equal and thus permit valid comparisons between relative changes in speed?" The answer to this question seems to be "no," when one takes into consideration the sensitivity of speeds to tubes and the unsystematic variation in reaction to various similar tube highway configurations.

In any event it would seem from the results reported here that research workers using pneumatic road tubes have the burden of proof to show that tube effect is not material under the specific set of conditions in their particular study or experiment.

FOOTNOTES

1 The reaction of *non-free-flow* vehicles to the stimulus of the road tubes may be magnified out of proportion owing to the sudden slowing, or brake light flashing of the vehicle ahead.

2 See "Determining the Existence of Tube Effects" for a complete discussion of these functions.

3 R. P. Shumate and Richard Crowther, *Variability of Fixed Point Speed Measurements*, The Traffic Institute, Northwestern University, 1959, pp. 83-85.

4 A complete discussion of the manner in which differentiation between open and concealed display is made is contained in "Making Observations."

SURVEY OF ROAD PATROLS AND OBSERVATION REPORTS

by A. F. Mayerhofer

A SURVEY was conducted among a representative cross section of Class I carriers regarding road patrols and the submission of observation reports, which included the top carriers listed according to current revenue earnings. Such information can be of importance to safety supervisors in order to evaluate their present programs properly and adequately and serve as a useful guide in revising present procedures and future plans.

With the exception of one carrier, all those surveyed maintained patrols of some type. The majority, therefore, are clearly of the opinion that road patrols are an important tool in driver control and consider such activity an important part of highway operations.

Many Titles for Job

The exact job description is by no means uniform and includes such titles as "Road Supervisor," "Safety Supervisor," "District Driver Supervisor," "Safety Engineer," "Field Safety Supervisor," "Driver Supervisor," "Driver Foreman," "Safety Director," "Field Supervisor," and "Operations Safety Supervisor."

Fifty per cent of the responding carriers utilized other than company road patrols for the performance of this function. It is also important to note that only half of the group used road patrolmen solely for the purpose of observing driver activities and driver control on the highways.

The preponderance of companies reporting indicated that the activities of the road patrolmen are under their immediate supervision, direction, and control. Approximately 52 per cent use other than company employees for patrol functions and about 38 per cent utilize both as a combination to serve this function.

The services being rendered by a vended agency in this regard were fairly well spread over the group and include such concerns as Rumsey Engineering Service, Motor Transport Safety Service, Truck Insurance Exchange, and one each subscribing to EL&N Service and National Safety Service. One carrier reported the use of two services as a supplement to company-operated road patrols, and another carrier reported the use of four services in a combination operating in various areas of their system.

With regard to the top group of major carriers, 54 per cent are using a vended service of some type, and 46 per cent are presently utilizing a combination of company-operated road service and contracted engineering patrols. The importance, therefore, of employing both in combination cannot be overlooked as an important feature of driver control. Approximately 45 per cent of this selected group are furnished patrol services by their present insurance carrier as a part of routine insurance coverage. About 50 per cent, however, report self-insurance and utilization of either company-supported patrols or combinations with vended engineering services under contractual arrangements. Of the entire group surveyed, only 40 per cent of the total are presently being provided insurance company patrol service.

Important Subject—Cooperative Road Controls

The important subject of cooperative road patrols was surveyed and several important conclusions may be drawn from this response. The vast majority (over 85 per cent) presently participate in some type of joint, cooperative safety patrol activity. This group looks upon such cooperation among the various truck lines with favor and is of the opinion that it can serve a most useful, supplemental purpose.

Several carriers, however, expressed the need for a better understanding in this particular area and stated that they

would recommend this practice with certain qualifications. It is apparent that unless such reports reflect an accurate picture of the alleged infraction, based upon the training and the experience of the individual conducting the road observation, they may be more detrimental than useful. It cannot be denied or overstressed in this report that such dangers apparently exist and that adequate training and proper indoctrination are prerequisites for effective road observation and reporting. It is most gratifying to note that over 60 per cent of the supervisors contacted in this study have issued instructions and directives to their present staff to contribute complimentary or courtesy reports for other truck lines, in case of serious safety infractions, as a routine function of road patrolling. It is our opinion that much progress could be made in this particular area of safety endeavor and should be promoted into a spot of major importance in the development of a system of national control. Such a system of continuous and extensive self-policing by the industry itself would pay handsome dividends to all concerned in accident prevention and public relations.

Auxiliary Patrols Needed

The need for auxiliary patrols may be realized when we become aware of an important factor—only 28 per cent of the total group participating in this survey employ personnel for the purpose of full-time road observation. Forty-eight per cent, however, report the use of some part-time, supplemental personnel utilized on a strictly limited basis.

The average number of company patrolmen employed for all carriers surveyed was three. This did not include the unusually large staff of 30 and 13, reported by two carriers among the top group, which are exceptions. Their salaries ranged from an average of $115 per week, or about $6,000 annually, among the intermediately-grouped carriers to an average of $125 per week, or an average of $6,500 annually, among the top carriers reporting. The salaries indicated for all carriers surveyed was ranged from a low of $4,000 to a high of $7,800. The average weekly salary reported by all carriers participating was $120. Three carriers employing road patrolmen declined to release these data on the grounds that such information is confidential.

The majority, or about 62 per cent, reported directly to the Director of Safety for supervision and direction. In several instances, the road patrol activities were supervised jointly by the Operations Supervisor and the Safety Director. Several carriers reported line supervision through the Director of Drivers. One carrier reported such activities were conducted under the direction of the General Operations Manager, and in another instance, the patrolmen were under the control of the District Supervisor. In general, however, the direction, supervision, and control, (and training) emanate from the Safety Department and its director.

The placement of the road patrolmen was also analyzed. The systems varied considerably. This covered both extremes—from several broad geographical divisions to no actual assignment where the patrolman is moved daily or weekly where needed.

In general, the geographical placement of personnel followed this pattern: Strategically placed in order to observe the greatest number of vehicles; by division, sections, districts, and then specific areas within the given district; territories, as East and West, Middlewest, South and East, or East, Central, North and West; or by routes or gateways, such as the "Northern Route" (between given points) and the "Southern Route," "Pennsylvania Turnpike System," etc.

The greatest number report that the patrolmen move about on the system, as needed by circumstances and receive no permanent or specific geographical assignment.

Several companies reported permanently assigned locations or territories within which there are designated and specific areas. The patrolmen reside permanently within such territory and do not leave the area unless required to do so by an emergency, special assignment, joint road block, etc.

Among the principal duties performed by the patrolmen were included such items of activity as terminal inspections, vehicle inspections and condition reports, driver training, accident investigation and cargo losses, inspection of city equipment, handling wrecks, such as removal of equipment and transfer of cargo; investigation of accident claims, inspection of tire stations, rest stops, restaurants, hotels and motels, examination of driver personnel records, safety meetings, driver recruitment, selection and placement, investigation of employee injuries, fire control, protection, and prevention, both on the highway and at terminals and other company facilities, transition training on new equipment, check-out of drivers and re-training, liaison between company and dealers and vendors, survey and establish routings, control tachograph charts, public relations, file weather and road reports, administer company roadeos, state roadeos, attend state association and other local organizational meetings, participate in joint road blocks and cooperative road patrols.

Interesting and Revealing Facts Disclosed

Some interesting and revealing facts were disclosed in surveying the approximate or estimated percentage of the patrolman's time spent in actually road checking drivers on the open highway. An average of between 40 to 50 per cent was established for the top group of carriers participating, and an average of between 50 to 60 per cent for the others. These percentages ranged from 5 per cent all the way up to 100 per cent so it is apparent that the function of the road patrolman varies considerably among carriers in both the top and intermediate groups. It would appear from this response that the larger the carrier the more varied, in general, is the field of the patrolman's activities. Seventy-two per cent of the truck lines rotate their personnel from one locale to another—from one district to another periodically.

Over 90 per cent furnish the patrolman with a company-owned vehicle. About 15 per cent use some privately-owned as well as company-owned equipment. Approximately 72 per cent favor the sedan over other body styles. Station wagons were preferred by the remainder with the rare exception of a few concerns which utilized panel

trucks, etc., 60 per cent had all vehicles identified. About 30 per cent use some identified and some unidentified cars in their road patrol fleet. Only one trucker equipped the vehicle with a fixed camera mounted on the fender of the car. The majority carry cameras.

Only one company reported that a given quota of observation reports was established for their road patrolmen. The remainder set no quota whatsoever. The average number of observation reports obtained per patrolman was ten per day or approximately 40 to 50 per week, or between 175 to 200 reports per month.

The number of reports being received by these carriers from vended services, insurance carriers, and cooperative road patrols was also studied. The following results were obtained:

The over-all average number of reports obtained from insurance company patrol services range from a low of ten reports per month to a high of 1,000. The average was set at 400 observations per month.

Cooperative observations ranged from two up to 75 monthly. The average number reported appears to range between 15 and 20 reports per month.

Reports from contracted or vended patrol services range from a low of 150 to a high of 1,000; the average ranging between 350 and 375 per month.

Considering the top group of carriers only, this may be broken down as follows:

	Range	Average
Insurance Reports	115 - 1000	550
Cooperative Reports	2 - 75	17
Vended Service	200 - 1000	450

Considering the intermediate carriers below the top group, this may be presented as follows:

	Range	Average
Insurance Reports	75 - 650	250
Cooperative Reports	5 - 50	24
Vended Service	150 - 400	260

Eighty-five per cent of all companies surveyed permit the vehicle to be stopped on the highway by both insurance, contracted, and company-operated patrol. This factor was not surveyed in regard to cooperative road patrols and no conclusions are available. It is a generally accepted policy in the industry, however, that this procedure is limited to contracted agencies only serving as agents of the carrier in this particular capacity, under a definite understanding.

Favorable Reports Presented in Various Ways

It was most interesting to note the various methods used by these carriers in the disposition of favorable observation reports. The majority file the record and no further action is taken.

In some cases the favorable report is posted prominently on the bulletin board for all others to observe. In several cases the record is submitted to the terminal manager supervising the driver and placed in the local terminal files, on a decentralized basis. Several other carriers publish the reports in their house organs. In one instance the carrier accumulates these reports and removes them from the file and interviews the driver annually. Another firm furnished the report with the driver's payroll check. In some instances the carrier allows the driver a copy of the record. Several companies forward the record to the driver's home through the mail. The important factors here appear to be the practice of maintaining files for favorable reports and bringing such reports to the attention of the employee, along with any unfavorable observations which may be made from time to time. Only two companies reported that the reports are destroyed upon receipt, thus indicating an interest only in the unfavorable reports being rendered. Ninety per cent of the reporting carriers identify the driver covered by the report whether it is favorable or unfavorable.

Handling of Violation Reports Studied

The matter of handling unfavorable, or violation reports, was carefully surveyed and studied. In every instance, the reporting carrier took some action regarding adverse reports. In 20 per cent of the group, the driver supervisor handled the entire procedure both by personal interview and the necessary recording, notices, etc. About 33 per cent depended upon personal interviews at the terminal or general office level, when possible, and 33 per cent rendered warning notices without personal contact. One carrier delegates this responsibility solely to the Superintendent of Drivers, through personal interview.

In another case, the carrier directs all violations to the driver's home by personal mail. Another firm reported that this duty is assigned to one person, the Personnel Manager, who takes the necessary action to have the man interviewed and warned through the terminal exercising supervision and control. The standard procedure appears to be a mixture of personal corrective interview, follow-up written endorsements and notices for the record, and necessary filings with local unions in accordance with contractual agreements and provisions, and a systematic effort to rehabilitate instead of separate. The use of the family and the driver's home environment cannot be over-emphasized at this point. Recognition, too, of all favorable performance is vital to the development of this program.

It is especially significant to note that 82 per cent of the participating carriers have discharged personnel on documentary evidence available through the road observation report. It is even more significant to note that 52 per cent of all carriers responding to this particular question have discharged drivers on evidence submitted through their insurance road patrols or contracted engineering services. Forty-eight per cent have declined to take separation action on the strength of this type report or have been completely successful in defending their position. There appears to be a considerable difference of opinion in this important area of driver control. Fifty-six per cent report definite union difficulties in this area.

The number of favorable reports received relative to the total number being reported ranges from 60 per cent up

to 98 per cent. The average number, however, for the entire group reporting, was 95 per cent. It is obvious from this record that the handling of favorable reports is most important in the interest of sound economy and efficiency and such records cannot be handled lightly. Ninety-five per cent of the net result of patrol activity, therefore, falls in this area, at a given unit cost to the carrier, and should be utilized completely in attaining better highway discipline through the positive approach of recognizing good road performance.

The additional duties of the road patrolman were investigated. Ninety-two per cent of the group require their men to make inspections and report conditions at local terminals and stations. Only 44 per cent are required to make spot checks or safety checks of equipment on the highway. Ninety-two per cent, however, are required to make vehicle inspections in terminal or station yards as a part of the routine terminal inspection.

An effort was made to correlate the number of drivers in the operation to the number of supervisors in use by the reporting carriers. This ranged from a low of 40 drivers per supervisor to a high of 1,000 drivers per supervisor. The general average for all carriers reporting was established at 150 men per road supervisor or road patrolman.

A further attempt was made to relate the number of route miles in the carrier's operation to the number of supervisors in use patrolling such routes, which was unsuccessful because of gross misunderstanding of a poorly-worded question submitted in the survey. The response in this regard was conflicting so that definite or useful conclusion could not be developed.

The next survey approach was aimed at obtaining a general opinion of the efficiency and economy of the various services available, and the usefulness of the records in driver control. This varied greatly as reflected by the following recapitulation of opinion:

CLASSIFICATION	1st	2d	3d	4th	5th	6th	RATING
Company Patrols	21	4	2	1	0	0	NUMBER
Contracted Service	5	13	5	2	0	0	
Cooperative Patrols	0	6	11	2	0	0	
Courtesy Reports	1	2	1	13	0	2	

Summary

In summary, it may be stated that this representative cross section of the trucking industry reflects the vital need for road patrols as an essential part of good driver control.

It would appear that the actual observation of drivers' performance is only a small part of the road patrolman's delegated responsibilities.

Vended services as a substitute for company-operated patrols seems to be well accepted by the top group of major carriers. Combinations of various types of patrol services which are presently available appear to be popular and necessary to those operations being conducted under a self-insurance program.

Cooperative road patrol services are being slowly developed from year to year and are steadily gaining favor with all carriers both major and intermediate. This area has a tremendous potential for expansion and usefulness. It has as its most powerful inducement the factor of economy. The need for supplemental or auxiliary patrols cannot be overlooked. About one-fourth of all surveyed use full-time patrolmen, and fewer than one out of five use any other supplemental personnel or devices. There presently exists a considerable range of the patrolmen's salary, clearly indicating the need for further industrial surveys and clarification in this particular job classification. The average salary would appear to be somewhat lower than the driver rate which obviously is not conducive to the attraction of the best material available from the driver groups.

Unidentified Vehicles Unpopular

The practice of equipping the employee with a company-owned sedan, properly identified, and movement of the patrolman from area to area as needed, seems to be generally accepted by the industry. Unidentified vehicles are not popular and where utilized are probably used for survey purposes only when the identity of the vehicle would hinder the accuracy of the study.

Demanding a given quota of reports is definitely considered a poor organizational practice and is frowned upon by most carriers. A satisfactory level, however, of the quantity being reported is soon established normally.

The stopping of vehicles on the highway by unidentified vehicles can precipitate an extremely hazardous condition and is unanimously prohibited. Stopping vehicles in a safe location off the highway and the berm of same by agreement of the carrier is sanctioned by four out of every five carriers surveyed.

The tendency to mishandle costly observation reports of a favorable nature was not too apparent among the participating carriers. Nine out of every ten identified and followed through on both favorable and unfavorable reports.

Four out of five carriers report successful and uncomplicated separation of recalcitrant personnel on the weight of observation reports by company patrols, where the driver has been stopped and identified and the provisions of the contract have been properly met. Resistance was encountered when such action is attempted by use of insurance or vended service reports. Over half, however, report discharges by utilization of vended service and insurance records when they meet the provisions of the contract. (Stopping and identifying driver, warnings, etc.)

Favorable reports reflect an extremely high degree of favorable road performance. The most popular combination for road observation is company-operated and vended (engineering service) patrolling.

Tachograph may be considered as a valuable supplement but cannot be substituted for road patrol service. Difficulty may lie in the accuracy of charts, calibrating, administration, and vulnerability of the instrument to damage, wear, and tampering. There is the tendency here to equip without fully understanding maintenance and control problems.